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Noyes's College Text-Book of Chemistry

By W. A. NOYES, Professor in the University of Illinois
336 pages of text. 12mo

This *College Textbook of Chemistry* is designed, more especially, for students of the freshman or sophomore years in college who have not studied chemistry in high school. It is considerably briefer than the author's previous *Textbook of Chemistry*, and its style is exceptionally clear and simple.

As with all textbooks for beginners, two purposes have been constantly kept in mind while writing the book: the presentation of a few of the multitude of chemical facts which touch our modern life, in such a manner that they can be clearly understood, and the discussion of the theories and principles around which all our chemical knowledge is grouped.

The teacher of chemistry is embarrassed by the vast and ever increasing amount of knowledge at his disposal and is often tempted to present many more topics than the student can possibly remember. In trying to avoid this difficulty many facts ordinarily included in an elementary textbook have been omitted and those which are given are brought as far as possible into close logical relations.

The summary at the close of each chapter is a somewhat unusual feature of the book. It is hoped that these summaries will be found useful.

Success in the study of chemistry depends especially on the ability to learn new facts in their relation to those which have already been acquired and on the cultivation of a logical as distinguished from an arbitrary memory. The exercises at the close of each chapter and questions occasionally inserted in the text are designed to assist the student in this direction.

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THE SCIENTIFIC MONTHLY

APRIL, 1920

THE BEGINNINGS OF HUMAN HISTORY READ FROM THE GEOLOGICAL RECORD: THE EMERGENCE OF MAN[1]

By Professor JOHN C. MERRIAM

UNIVERSITY OF CALIFORNIA

PART TWO

GEOLOGICAL HISTORY OF MAN

HAVING considered the biological position of man and the possibility of his ancestry leading out of the evolution of presumably inferior, and certainly older primate groups, we are in a favorable position for discussion of the earliest evidences of man himself; but before entering upon consideration of the specific problem of human history, it is well to bring to mind certain of the most fundamental conclusions obtained in an inspection of the wide field of history of other living creatures. To one who reads this story, the evidence of the geologic and paleontologic series amounts to a demonstration that our available records of life in the broadest view count up to many tens of millions of years at the least, and possibly reach to hundreds of millions. We see also that in this period the record shows life to have been in an almost continuous state of change; that the life of each period exhibits closer resemblance to that of the periods immediately preceding and following than to the more remote divisions of time; and that series of forms with certain common characters but differing in grade of specialization generally tend toward greater specialization from earlier to later time. The manner in which the modifications take place may not always be understood, and the paleontologist may admit his ignorance of the causes, but the evidence of continu-

[1] Delivered before the National Academy of Sciences in April, 1918, as the sixth series of lectures on the William Ellery Hale Foundation.

ing change and advancing specialization in series of presumably connected or related types seems reasonably clear.

Well within the generally accepted record of the geologist, and forming a part of the life succession of the paleontologist, there appears the human element, taking its place in the life sequence and raising, whether we desire it or not, the question of man's inclusion in the general scheme of evolution, or if his relation to the other biological series, for which we have traced a long history and through which we seem to see running the thread of continuity.

From what we have seen in our paleontologic record there is every reason to believe that there was a long period in which generalized members of the mammalian group were widely distributed and man had not yet appeared. Unless we take the view that man is the product of special creation, built upon lines similar to those of other organisms, we naturally assume that his origin is found in the earlier less specialized type, and by way of an evolutionary process not unlike those that have determined the development of other mammalian forms by modification of types already existing.

The Earliest Remains Referred to the Human Type

Discovery of Pithecanthropus

For the present we shall consider only those relics of man which furnish the earliest known record of the existence of our race upon the earth, leaving for a later paper an account of the succeeding stages represented by the cave men and their immediate predecessors.

Probably no paleontologic contribution published has furnished the basis for more extended and more critical discussion than the paper of Eugene Dubois published in 1894, describing a remarkable tooth, a skull-cap and a thigh bone, found in deposits of considerable geologic antiquity on the island of Java. These specimens presumed to represent a single type, and probably one individual, were first referred to by Dubois in 1892 as *Anthropopithecus erectus* or the erect man ape, assumed to represent a transition between man and the apes. After having with most commendable patience investigated his material for three years, Dubois published his now classic memoir. In this paper the specimens were referred to as *Pithecanthropus*, or ape man, this generic name having been used by Haeckel in 1868 for a hypothetical creature assumed to walk erect, and to have higher mental development than the anthropoids, although not yet attaining to the formulation of speech.

FIG. 1. Locality at which *Pithecanthropus* Remains were discovered on the Bengawan River in Java. After L. Selenka and M. Blanckenhorn.

Following this work of Dubois, extensive excavations initiated by Frau Selenka were carried on in 1907 and 1908 on the site of the occurrence of the type specimen. These important studies added much to our knowledge of the formation in which the Dubois specimen was found, and of the fauna associated with it, but did not contribute additional *Pithecanthropus* remains.

OCCURRENCE AND ASSOCIATED FAUNA

The specimens referred to *Pithecanthropus* all exhibited approximately the same mode of preservation, which is similar to that of the extinct types of mammals found in the same strata, and which have clearly been buried in process of accumulation of the formation. There was no evidence indicating intrusion or burial of these remains in the formation since accumulation, and the relation of the Dubois specimen to the stage of geologic time represented by the deposits containing the mammalian fauna is not seriously questioned.

Some doubt has been expressed, however, whether the skull-cap, the femur, and the teeth belong to the same individual. The first tooth and the skull-cap were found in 1891 only about three feet apart. The femur, discovered the following year, was situated nearly fifty feet from the location of the skull. Inasmuch as the femur is by some considered quite human in aspect, while the skull-cap is assumed to be pre-human, question has naturally been raised as to the relation of these elements to each other. On the other hand, the fact that skull, teeth and

thigh bone occurred approximately together, with similar mode of preservation, and with peculiar characters differing from those of all known anthropoids and humans, is considered by many as evidence distinctly favoring the view of Dubois that the specimens all represent one species and one individual. It will evidently not be possible to settle this question with full satisfaction until further occurrences of remains of this most interesting type are discovered.

The *Pithecanthropus* specimens were found in a formation composed in large part of volcanic tuff. It was considered by Dubois as a river accumulation and was assumed by him to

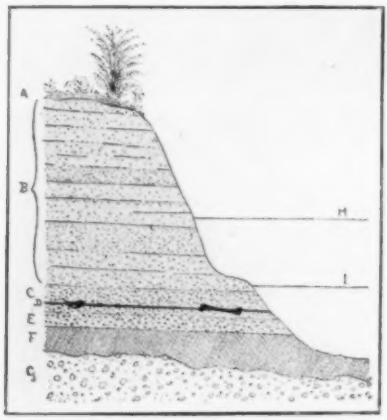

FIG. 2. Section of Strata at the Locality where the *Pithecanthropus* Bones were discovered. *A*, area of growing plants; *B*, soft sandstone; *C*, lapilli stratum; *D*, level at which the skeletal remains were found; *E*, conglomerate; *F*, argillaceous layer; *G*, marine breccia; *H*, wet-season level of the river; *I*, dry-season level of the river. After E. Dubois.

have a total thickness of over 1,000 feet. The results of the Selenka expedition indicated that the so-called *Pithecanthropus* layer, containing bones of many extinct animals, owes its origin to the gradual working over of ash beds formed through eruption of great volcanoes near at hand. In the course of the eruption many animals were destroyed and the remains buried in ashes. Rain wash worked over the ashes and the entombed bodies of animals. The bones washed out were later accumulated at points where swamps or comparatively level stretches in the stream stopped transportation of heavy objects by slowing of the current.

The particular layer in which the *Pithecanthropus* specimens were discovered, was found by Dubois and by the Selenka expedition to contain a large number of remains of mammals.

representing at least thirty species. These include the pre-elephant, or *Stegodon*, which is the immediate ancestor of the modern elephant, also cats, hyenas, rhinoceros, tapir, pig, hippopotamus, deer, buffalo and monkeys. Along with the mammals was a considerable variety of reptiles, among which were crocodiles, gavials, lizards, snakes and turtles.

The mammal fauna of the *Pithecanthropus* beds resembles in many respects that of the Java region of the present day, but practically all of the species are extinct. Certain forms, as *Stegodon*, are generally characteristic of the Pliocene, the second period preceding the present. The mammal assemblage as a whole is very close to that of southern Asiatic formations representing a stage near the close of the Pliocene, as seen in certain of the later levels of the Siwalik series in India. As mammal faunas are generally short-lived in the geologic sense, there seem good reasons for considering that the beds containing *Pithecanthropus* and the associated mammal remains were deposited at a period approximately the same as the late Pliocene on the early part of the next stage, the Pleistocene as recognized in other formations of which the age has been clearly determined (see Fig. 7).

Suggestions regarding age of the beds have also been made by Blanckenhorn on the basis of climatic conditions obtaining during accumulation of the deposits. Abundant plant collections secured in the formation containing *Pithecanthropus* indicate to practically all investigators who have examined them a much more humid climate than that now found in this region, and perhaps a temperature five or six degrees colder. Considering the Glacial Epoch to have been a time of relatively high humidity, Blanckenhorn believed the available evidence indicative of a time stage for the *Pithecanthropus* beds corresponding approximately to the first or Günz stage of glacial time, or near the beginning of the Pleistocene period.

NATURE OF REMAINS

Study of the remains of *Pithecanthropus* has been carried to an extreme of detail rarely reached in investigation of other known specimens. It is only regrettable that this work, so far as it has been done by workers other than Dubois, has been based largely upon casts of the originals rather than upon the specimens themselves.

The *tooth* first found is of extraordinary size, and quite different from the corresponding third upper molars of human

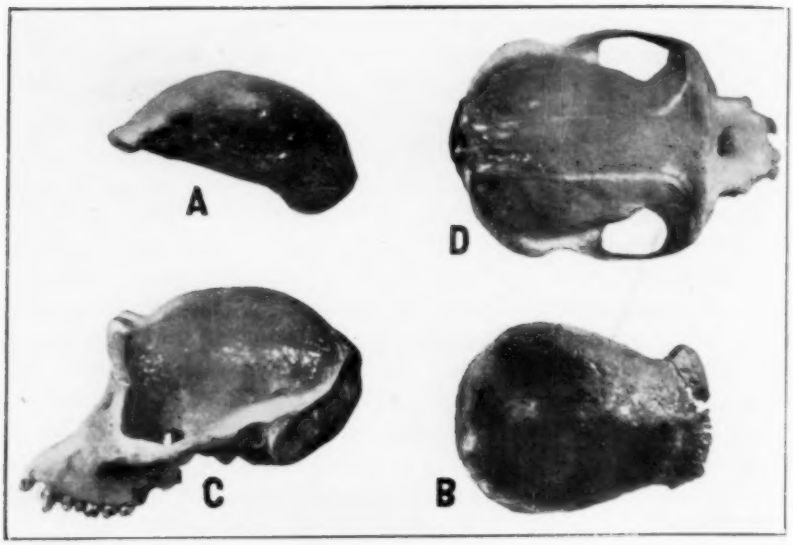

FIG. 3. *Pithecanthropus erectus*, Skull cap Compared with Skull of a Modern Chimpanzee. After E. Dubois. *A*, *Pithecanthropus* skull cap seen from the left side, ×1/3; *B*, same specimen seen from above; *C*, chimpanzee skull from the left side, ×4/9; *D*, skull seen from above.

beings. It either represents a large individual, or an individual with relatively large and heavy teeth.

The *skull-cap* has been examined from almost every possible angle of physical or mental vision by a great group of investigators, and is by common consent a most unusual type, bearing in general the characters of a human being, but more beast-like than any human species recent or extinct. The vault of the cranium is low, and the arch is flat both on the forehead and on the back of the skull. The eyebrow ridges are very prominent, as in the apes, and the skull is narrow behind the eyes. The brain capacity is lower than that of normal human types. Measurements of the cranial capacity given by Dubois have approximately 900 cubic centimeters, while the corresponding measurement of anthropoids rarely reaches 600 c.c., and of male Caucasian humans approximately 1,500 c.c.

From the interpretation of this specimen, it is evident that *Pithecanthropus* represents a type previously unknown in either the human or the anthropoid group. It differs from the apes in its larger brain capacity coupled with reduced musculature relating to the jaws. It differs from man in its lower brain capacity and more beast-like contour of the skull. If there is justification for the assumption that the large teeth pertain to

the same individual as the skull-cap, there seems sufficient warrant for accepting an ape-like reconstruction of the face with the large protruding jaws needed to support large teeth.

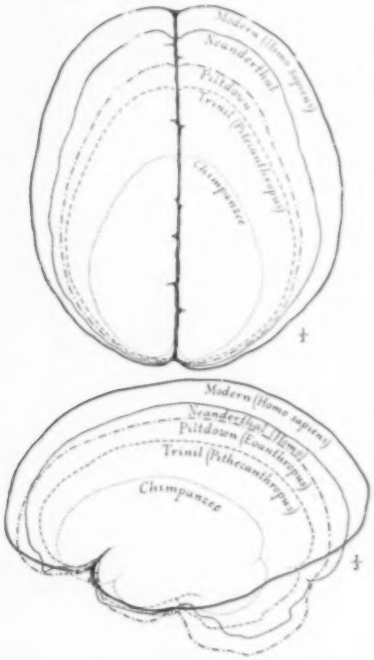

FIG. 4. Diagram showing Outlines of the *Pithecanthropus* Brain compared with that of the Chimpanzee and Certain Human Types. Upper figure, outline seen from above; lower figure outline seen from side. After Osborn, "Men of Old Stone Age," Charles Scribner's Sons.

Many restorations of the skull have been made, among which one of the most interesting is that by Dubois, showing

FIG. 5 Reconstruction of the *Pithecanthropus* Skull, ×1/3. After E. Dubois.

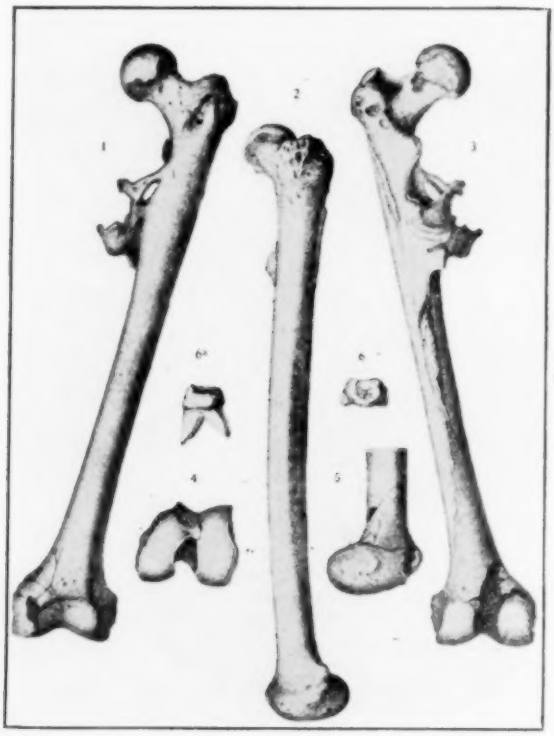

FIG. 6. Femur and Tooth of *Pithecanthropus*. 1, Femur from above; 2, from side; 3, from behind; 4, from below; 5, lower end from median side; 6, Right third upper molar from below; 6*a*, from behind. Much reduced. After Dubois.

the low-vaulted skull, prominent eye ridges, heavy jaws, and protruding facial region.

The *thigh bone*, found at the same level with the skull, resembles the corresponding bone of typical humans in some respects more closely than the skull resembles the cranium of human types. It approaches the human femur and differs generally from that of apes in its length, slenderness and straightness of shaft, as in other minor details. It is in fact more slender in some respects than the thigh bone of modern races. Resemblance to the femur of modern man is not, however, in any sense complete. Dubois has called particular attention to the more nearly circular cross-sections of the bone, and to distinct differences in certain important areas of muscle attachment. To a slight extent the peculiarities of the *Pithecanthropus* femur resembles those of the gibbon, but they do not correspond fully to gibbon characters.

This thigh bone cannot conceivably belong to an animal of

the ape type. The form of the shaft and the nature of the articulations at the distal end are different from these features in apes. They show that the animal evidently walked with a degree of erectness comparable to that of typical humans, and that it must have possessed a length of limb and extent of stride in locomotion comparable to that of modern forms. This character of specialization for running or walking is not naturally combined with specialization of the anterior limbs for climbing, and we may safely assume shortness of arm like that of humans, and different from the type of limb in arboreal anthropoids.

Dubois and others have considered that the degree of deviation in form of the femur away from that of normal modern man and from the apes corresponds approximately to the extent of difference between the skull-cap of *Pithecanthropus* and that of both typical apes and typical men.

SYSTEMATIC POSITION

To the gratification of a large group of scientists, not long after publication of his memoir on *Pithecanthropus*, Dubois appeared with his specimens before the Zoological Congress at Leyden, where there occurred one of the most interesting discussions on the history of man that has ever taken place. Divergence of opinion as to significance of the remains lay largely in the question of classification. Five of the noted authorities, including Virchow, considered the animal to represent an ape; seven, including Cunningham, Lydekker and Topinard, considered it a man; and seven others, including Dubois, Manouvrier, Marsh and Haeckel, considered it a transition form.

With the wide knowledge of structure of man and of apes possessed by all of the eminent students whose opinions were expressed, it is clear that the essential difference lay in the definition of diagnostic characters of humans and apes. It is also evident that opinion of the group as a whole indicates the intermediate position of Dubois's find between typical humans and typical anthropoids. Centering upon the controversy over Dubois's specimens there has developed a discussion as to definition of hominid and anthropoid characters. There seems now to be practical unanimity of view that the essential distinctions lie in possession by man of somewhat greater brain capacity, erect position, length of limb, freedom of arm and hand from locomotion, and perhaps in possession of speech with sentence construction. These characters are contrasted with

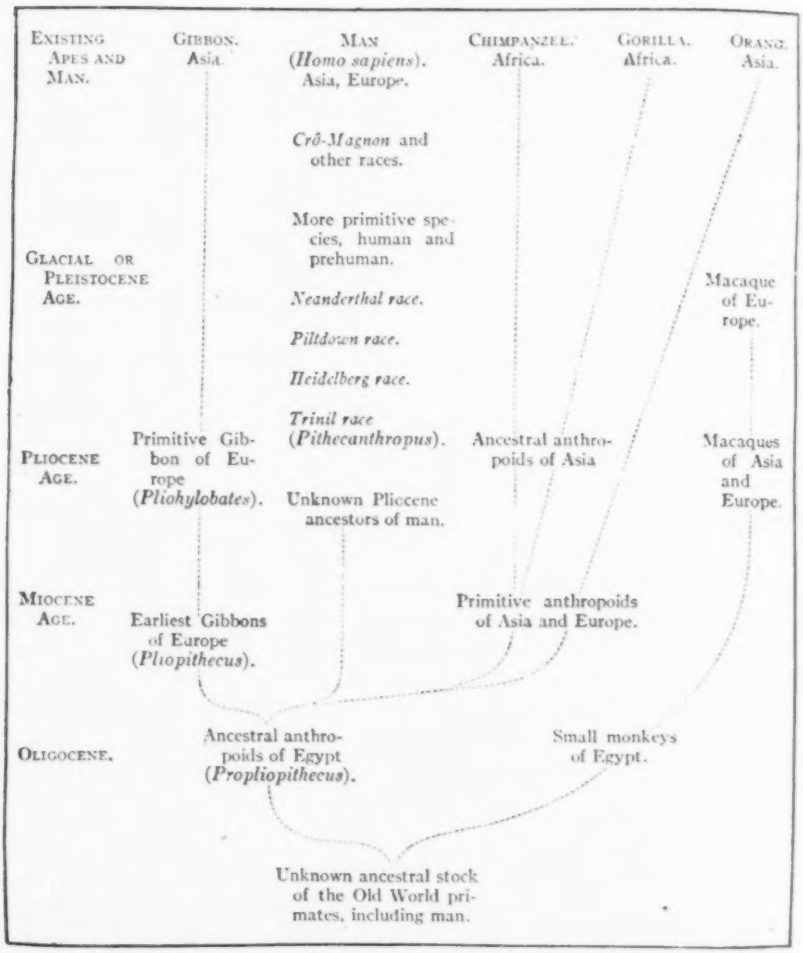

Fig. 7. Outline of the Evolution of Anthropoid Apes and Man. After Osborn. "Old Stone Age," Charles Scribner's Sons.

the smaller brain, more prominent face, relatively short grasping posterior limbs, and long anterior limbs used for climbing, seen in apes. Judged according to this definition, it is evident that Dubois's specimen represents a stage just above the middle territory between man and the apes. The animal seems particularly human in its limbs and pre-human, with strong anthropoid tendencies, in the skull.

RECONSTRUCTION

A reconstruction of the skeleton based upon skull-cap, teeth and femur of *Pithecanthropus* makes necessary the organiza-

tion of an animal with upright position and long straight limbs. There can be little doubt that with this structure of limb the foot must have been much as in modern man and the great toe much enlarged. With such limbs, the anterior extremities would be released from the burden of work in locomotion and come to serve the head alone. The large teeth make heavy jaws and a prominent face almost a certainty. The small brain case and prominent rims of the orbits give the combination of pre-human and super-anthropoid characters required by the long-sought missing link. If, as Dubois assumes, we are correct in relating these three classes of characters in one skeleton, there is little required to give the picture of a transition form as visualized by those who have foreseen the discovering of an ancestor of man coming out of the animal world.

FIG. 8. Reconstruction of *Pithecanthropus* according to the Belgian Artist Maseré, under the direction of Professor A. Rutot. After Osborn, "Old Stone Age," Charles Scribner's Sons.

FIG. 9. Reconstruction of *Pithecanthropus* from the Restoration modeled by J. H. McGregor. After Osborn, "Old Stone Age," Charles Scribner's Sons.

Although the remains of the Java man are most unfortunately fragmentary, it is worthy of note that the long discussions which have focused on these specimens have left us with a remarkable unanimity of opinion as to the nature of the type. It is one of the most significant facts in the whole range of paleontological study, that these earliest known remains referred to the human group seem without question to represent the type farthest from modern humans and nearest

to the anthropoids. The gap between the modern man and the modern apes is not fully bridged. It is perhaps worth stating that it will be difficult to define "fully bridging." The views as to what would constitute transition or "exactly intermediate stage" would probably vary with different investigators. It is, I think, quite certain that for the particular stage of geologic time in which it lived *Pithecanthropus* comes as near as we could well imagine to the expected intermediate form.

The Heidelberg Jaw

Occurrence and Associated Fauna

The second and perhaps the more important find of the two earliest known occurrences of remains referred to the human group is that known as the Heidelberg jaw. This specimen was obtained by Schoetensack, as the reward of twenty years persistent and continuous effort to learn whether traces of man might be found associated with the remains of extinct mammals at a locality in the Elsenz Valley not far from Heidelberg. From a sand pit Schoetensack had obtained many remains of extinct mammals of a stage sufficiently late in geological time to come within the assumed period of human evolution. Schoe-

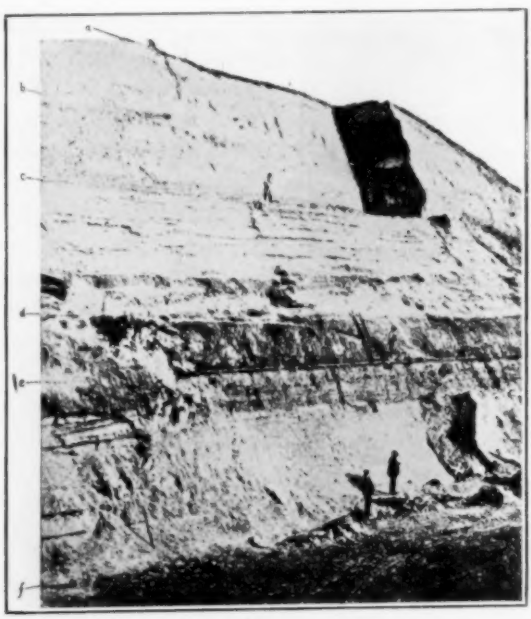

Fig. 10. Locality at which the Heidelberg Jaw was found, at a Depth of 79 feet below the Surface. The white cross in the lower right hand corner of the picture indicates the occurrence of the jaw. After O. Schoetensack.

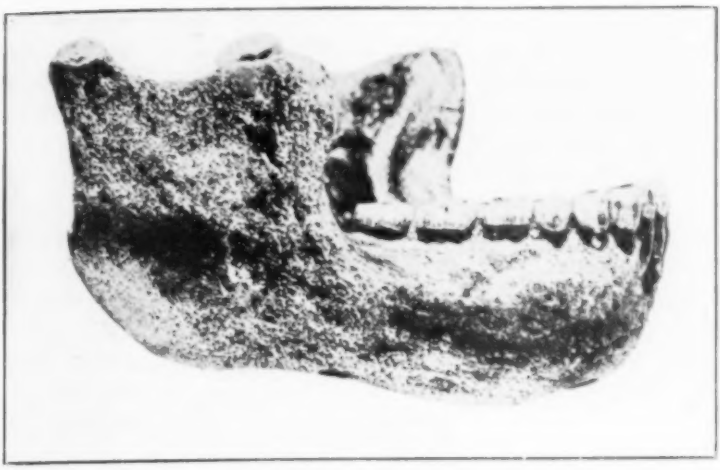

FIG. 11. Lateral View of the Heidelberg jaw, approximately two-thirds natural size. After Schoetensack.

tensack had repeatedly impressed upon J. Rösch, who worked at the pit, the importance of a possible find of human remains, and with the desirability of immediate and careful handling of any specimens suggesting man, should they appear. When in 1907 one of the workmen shoveled out a jaw of human type from a depth of approximately seventy-nine feet, the arrangements of long standing were favorable for rapid and trustworthy determination of the conditions of occurrence of this, one of the most remarkable known relics furnishing information bearing upon early human history.

The pit in which the famous jaw was found is cut into deposits consisting mainly of a formation known as the Mauer sands. At this locality the beds are about eighty-two feet in thickness. Between twenty-five and thirty feet of the upper portion of the deposits represent two formations known as loess. Below this upper section are the Mauer beds consisting mainly of sand and gravel. The human jaw was found only a little less than three feet from the base. The layer in which it was discovered was mainly gravel which was found to contain remains of extinct mammals characteristic of the Mauer beds. The condition of the undisturbed layer from which the jaw was taken leaves no room for doubt that all of the fossil remains, including the human jaw, were deposited at the same time with the sand and gravel making up this lowest portion of the formation.

The fauna found in the formation comprises not less than fourteen mammalian species, including the Etruscan rhinoceros, a species of horse, a primitive elephant (*Elephas antiquus*), a large lion (*Felis leo fossilis*), and remains of bear, deer, pig, and bison.

Schoetensack held that the Mauer fauna showed relationship to the early glacial or inter-glacial forest bed of Norfolk, as also to the Pliocene. The rhinoceros and the horse were considered especially significant of the earliest Pleistocene or Pliocene. Schoetensack placed particular stress upon the possibility that the jaw was the earliest authentic skeletal representative of the human group. Other investigators have considered the formation in which the Heidelberg jaw was found as considerably later and representing the second or Mindel-Riss inter-glacial stage.

DESCRIPTION OF REMAINS

The Heidelberg jaw undoubtedly constitutes one of the most interesting remains of early man thus far discovered. In comparison with all modern human types, it is characterized by its unusual size, lack of protruding chin, great strength and thickness of the body of the jaw, and unusual width of the area for the attachment of the muscles used in mastication. It is also characterized by the extraordinary form of the inner side of the anterior region of the jaw in an area marked by the attachment of certain muscles having an important relation to movement of the tongue in speech. The teeth are large, but not relatively large compared with the size of the jaw, and are notably human in practically all characteristics. While the jaw without the teeth might not have been called human, the teeth without the jaw would certainly have been assumed to represent a human type not differing greatly from known species.

The teeth are characterized by strength of the roots and large size of the pulp cavities; but considering their relation to the peculiar primitive type of jaw in which they are situated, the most striking characteristic of the dentition is probably the small, distinctly human canine. Comparison of the human dentition with that of the apes brings out the fact that humans are distinguished by limitation of the canines to such dimensions that the crowns of these teeth show approximately even length with those of their neighbors. In the apes, as in many other groups of mammals, the canines are very prominent, projecting far beyond the level of the neighboring teeth, and having an

important function in tearing food or in fighting. It has been assumed that primitive humans and intermediate types between man and other mammals would naturally be characterized by somewhat larger canines than those of modern species. In a jaw showing the massiveness, strength and primitiveness of the Heidelberg specimen, it would be natural to expect a canine of somewhat greater size than that of modern human types, whereas, as indicated, this tooth is here reduced to dimensions comparable to those of advanced modern types. It seems, therefore, that reduction of the canine to dimensions comparable to those of other teeth in the front of the jaw is one of the most significant characteristics of the early human type.

As shown in comparison of an Eskimo, the Heidelberg jaw, and a chimpanzee, the combination of characters in the Heidelberg specimen is more like that of the anthropoids than is the combination known in other forms referred to the human group. While the dentition in general is distinctly human, the size of the teeth, compared with those of many modern types, with the heavy roots and wide pulp cavities present characters which point toward an ancestral type resembling the anthropoids in some measure.

FIG. 12. Upper and Lower Views of Heidelberg Jaw, slightly exceeding one half natural size. After Schoetensack.

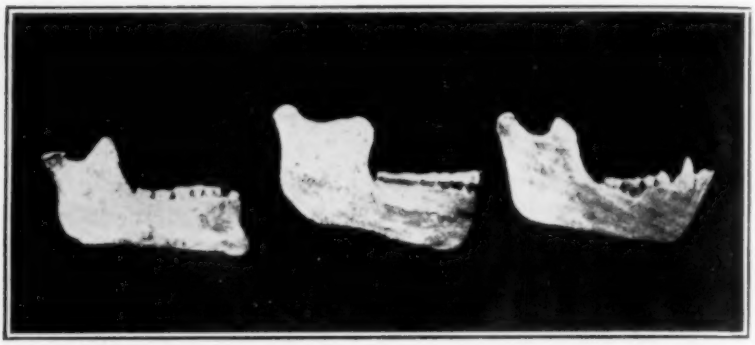

FIG. 13. Comparison of Heidelberg Jaw, in the middle of the picture, with a Modern Anthropoid to the right and an Eskimo to the left. After Osborn, "Old Stone Age," Charles Scribner's Sons.

In the configuration of the inner side of the jaw, there are strong evidences indicating that the musculature was different from that of modern man endowed with peculiar characters of the tongue related to speech.

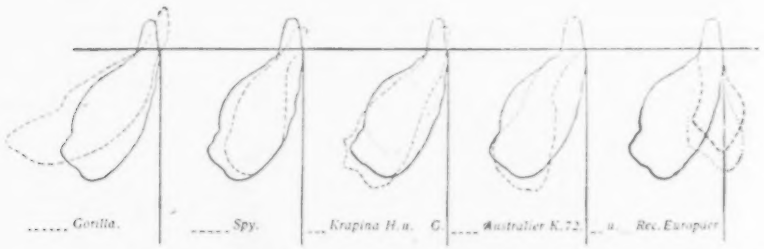

FIG. 14. Comparison of Vertical Sections through the Chin Region of the Lower Jaw. The sections of the Heidelberg Jaw are drawn in heavy line. The jaws with which comparison is made are indicated in broken lines.

The upper dentition and skull of the Heidelberg man have been reconstructed with great care by Dr. J. H. McGregor, and show a form resembling that of *Pithecanthropus* in many respects, but somewhat less ape-like. The teeth are smaller than those of *Pithecanthropus*. The face was probably less prominent and the brain capacity presumably greater.

SIGNIFICANCE OF THE HEIDELBERG JAW

The stratigraphic position of the beds from which the Heidelberg or Mauer jaw was obtained has not been questioned. The associated fauna has been known at a considerable number of other localities, so that the occurrence of this specimen is defined as that of a form living in middle or early Pleistocene

time, with certain suggestions of Pliocene stage. The age of the *Pithecanthropus* specimen has been the subject of much discussion, and is considered by some to represent a stage only a little earlier than that of the Mauer jaw. The association of the *Pithecanthropus* skull-cap, teeth, and femur as parts of one individual or one species has also been questioned. While it may be that the Java specimens represent the same individual, it is also within the range of possibility that they represent two or three distinct types. Though great interest at-

FIG. 15. Restoration of the Heidelberg Man by the Belgian Artist Maseré under the direction of Professor A. Rutot. After Osborn, "Old Stone Age," Charles Scribner's Sons.

taches to the discovery of *Pithecanthropus*, and to the speculations concerning the nature of the creature or creatures represented by this find, it is possible that the unquestionably primitive Mauer jaw, occurring in undoubted association with an early Pleistocene fauna and in a somewhat more definite geological situation, represents the most important and perhaps the earliest of the known human remains.

POSSIBLE TRACES OF ARTIFACTS PRODUCED BY EARLIEST KNOWN HUMANS, THE EOLITHIC CULTURE

Assuming that two of the essential characters separating the hominid group from the anthropoids are found in the free-

SYSTEM OF CHRONOLOGY FOR THE STONE AGE
(ADAPTED FROM RUTOT)

			RECENT			NEOLITHIC	*Recent fauna.*	Robenhausian. Campignian and Tardenoisian. Tourassian.
QUATERNARY				FOURTH GLACIAL PERIOD	Retreat.	PALEOLITHIC PERIOD	*Fauna of the Reindeer.*	Tarandian Industry.
					Advance.			
				THIRD GLACIAL PERIOD	Retreat.		*Fauna of the Mammoth (Elephas primigenius).*	Eburnean Industry.
					Advance.			
				SECOND GLACIAL PERIOD	Retreat.			Mousterian Industry.
					Advance.			Acheulian Industry. Chellean Industry. Mesvino-chellean or Strépyan Industry (Strépy, Belgium).
				FIRST GLACIAL PERIOD	Retreat.		*Fauna of Elephas antiquus.*	Mesvinian Ind. (Mesvin, Belgium). Reutelo-mesvinian or Mafflean Industry { Maffle near Ath, Belgium.
					Advance.			Reutelian Industry (Reutel, Belgium).
TERTIARY		PLIOCENE			Upper.		*Fauna of E. meridionalis*	Ind., Cromer Forest Bed (Norfolk). Industry of Dewlish (Dorset). Industry of Saint-Prest (France).
					Middle (Glacial.)			Industry of the Chalk Plateau (England).
					Lower.			
		MIOCENE			Upper.		*Fauna of Dinotherium*	Industry of Puy-Courny (France).
					Middle Lower.			
		OLIGOCENE			Upper.		*Fauna of Anthracotherium*	Industry of Thenay? (France).
					Middle Lower.			
		EOCENE						

FIG. 16. System of Chronology in the Stone Age. After MacCurdy.

ing of the anterior extremities to serve the head and in the development of intelligence to make possible a wide variety of uses for the anterior extremities, one naturally considers the making of implements or artifacts as a characteristic which should be distinctive of humans. Question has therefore arisen whether the *Pithecanthropus* and Heidelberg types had reached a stage at which natural objects were regularly worked into artificial forms.

The Selenka expedition secured along with other fossil remains found in the *Pithecanthropus* beds of Java, a considerable number of bone fragments showing peculiar fracture and worn points suggesting use by primitive man. Blanckenhorn and others who examined these specimens carefully were, however, of the opinion that they might have been produced by fracture of bones carried in a stream or broken by beasts, and that they were not certainly to be attributed to the work of the hominid represented in these strata.

By far the largest representation of evidence suggesting the use of implements by primitive man-like forms near the period of transition from ape to man, is that furnished by the great quantity of flaked flints obtained from strata ranging from Pleistocene down through the late and middle Cenozoic formations of western Europe. These specimens, representing what have been assumed to be the first artificial implements, have been designated as *eoliths*, or the work of the dawn period of implement making. Especially through the work of Rutot,

FIG. 17. Eoliths from Belgium. Approximately three-fifths natural size. After MacCurdy.

a considerable variety of flint forms coming from deposits ranging back through the period of *Pithecanthropus* and the Heidelberg man to older time stages have been interpreted as early efforts at preparation of artificial tools.

It is to be assumed that the earliest stone implements used by man were fragments which in their unmodified form were somewhat better fitted for the use of his hand than other more unwieldy or irregular pieces. Presuming that man early learned the cutting power of a rock, it is probable that fragments with sharp edges naturally produced were used at times in place of those with rounded contour, and that specimens thus used and fractured by use may have been found to have an advantage over weathered rocks naturally broken. This may have led to artificial preparation of such broken stones. There should, therefore, be an intermediate stage between unworked natural objects and the earliest tools purposely produced. From the materials representing this type, it would be exceedingly difficult to obtain evidence as to the way in which the objects were formed.

FIG. 18. Section from the Thames to Oldbury Hill near Ightham Kent, England. After MacCurdy, adapted from Prestwich. 1, Tertiary; 2, Chalk; 3, Upper Greensand and Gault;; 4, Lower Greensand. *a*, Red clay with flints—eoliths. *b*, Upper valley gravel—palæoliths. *c*, Low-level valley gravels—palæoliths.

No scientific controversy in Europe in the last decades has been more warmly contested than that over the artificial or non-artificial character of the eoliths or broken flints from formations of approximately Pliocene age, and in the zone in which one might expect the beginnings of implement making.

Among classic localities for these flints is that of the Kent Plateau, where, near Ightham in the south of England, Mr. Benjamin Harrison through many years has collected a wide range of flint forms from deposit assumed to be of Pliocene age. These eoliths occur in a red clay formation which lies across the edges of several upturned formations of considerable antiquity. Through this plateau there has been cut the valley of the Thames River. In sculpturing out the modern topography, various stages in the cutting of the valley along the streams have left terrace deposits on the banks, in which considerable numbers of flint implements have been found. In the lower

levels bordering the valley, the flints represent stages in the history of implement making corresponding to the middle Pleistocene of western Europe. In the upper levels representing the stage before the cutting of the valley, eoliths are obtained and the Pleistocene implements are not represented. There is, then, a historic sequence in the implement making of this region beginning with the stage of the eolith of the upper plateau, and grading through the more advanced stages of implements in the lower terraces.

The eoliths of the Kent Plateau correspond to a period not later, and probably earlier, than that of *Pithecanthropus*, and certainly older than that of the Heidelberg man. It is not improbable that flaked flints of this nature were used by both of these primitive human types. Whether the flints of the Kent Plateau were implements, and whether their origin was through artificial human production is difficult to determine. The fact that they grade insensibly into flaked flints of a type which may have been produced without the intervention of man, does not prove that their form was not due to human influence, and that they were not actually used by man. On the other hand, there is much evidence to indicate that many of the flints of these eolith beds may have been formed without human assistance.

Conclusions Regarding Significance of Earliest Hominid Remains

In concluding this phase of the discussion relating to the earliest traces of beings of our human group, it may be desirable to view in the large the data thus far assembled.

We have seen that remains of man-like forms are not limited to deposits of the present period nor are they found in all formations of the geologic succession. We find the earliest evidences indicating the presence of man occurring at that particular time, and not before the time, when evolution of the group of animals most closely resembling us had reached a stage near to the human type.

We find the earliest humans represented as a part of the normal life of the earth in a period so remote that in our calculation of its date a thousand years seems only as a day. *Pithecanthropus* and the man of Heidelberg were long dead, and had become ashes of a bygone age, before the world saw the beginning of many extended series of events, which changed the form of continents, shifted the earth climate back and forth from arctic to temperate in changes of the Glacial Epoch, and

passed over the earth long processions of living generations each in turn enjoying its hour of geologic time and fading out into the night of history. Although of these most ancient humans the earliest preserved traces are very faint, we find them adequate to show that at the time in which they lived our race was represented by beings set off from other primates by their erect bodies, long-striding limbs, hands that were free to build, and a brain that began to plan. And yet we find these forms more beast like and more anthropoid than any type of man at any later time.

So far as our evidence goes, it meets the requirements of those who assume the emergence of man from the animal world in the same manner in which innumerable other organic types have arisen in the long life record as we know it.

The reading of the full story of the advent of man is still to come, and the task of deciphering it will go on into coming centuries. With this must proceed also interpretation of the subsequent stages of man's growth or evolution, of which a brief account will be presented in a later paper.

THE MEASURE OF EXCELLENCE IN SCIENTIFIC ACTIVITY

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THE fundamental scientific activity is that which is expended in the search for truth, in discovering and establishing what can be made sure by experiments or by undisputed logical processes convincing to all who understand their nature. But truth has value only as it exists in the minds of men and women who procure from it practical or ideal benefits for themselves or others. Consequently scientific workers must see to it that a suitable portion of the body of truth is made widely known and is preserved for future generations. This calls for the work of the teacher and for the publication of the results of research. It also demands a certain measure of educational propaganda, at least in a country where scholarly traditions are just being developed, in order to create and maintain the proper interest in scientific truth and an appreciation of its values. Let us consider first this essential secondary part of scientific activity, the part which has to do with the dissemination of results once attained.

In a country in which research is as new as it is in ours and in which no controlling spirit of tradition is in existence for the maintenance of ideals, there is danger of thinking too lightly of breaking up the research work of an active man by assigning to him editorial duties in connection with periodicals both for the record of scientific achievement and for the work of educational propaganda. Committees of scientific organizations charged with the duty of procuring a man or a group of men to do such editorial labor are inclined to place an undue emphasis upon the work for which they are temporarily responsible and to seek appointees for it without a sufficiently keen realization of the needs of science as a whole. Unfortunately it sometimes requires actual experience with editorial work for a research man to realize how fundamentally it will break up the continuity of his thinking; and through this failure to see the true situation beforehand he may enter upon a labor in which he will be compelled to look upon himself as one of the principal offenders in the development of a spirit of thinking lightly of breaking up research work, although there is nothing in which he believes more ardently than in the value of ascertaining new truth.

The aptitude required for research and that for editorial labors are distinct in their character and there is no reason to believe that the possession of the one implies that of the other. In fact it is rare to find the two prominently present in the same individual. There are two types of things to be accomplished and two types of men from whom are to come the persons to accomplish them. In advancing a matter of secondary importance, essential though it is, we should be careful not to hinder the more vital work of research. Neither of these two things can be separated from the general cause of science. Moreover, we must keep in mind also the welfare of the individual who is asked by his colleagues to undertake any particular definite piece of work. To be sure, the cause of science is of sufficient importance to justify a considerable sacrifice on the part of one person in order that the general cause may prosper to the best advantage, unless that person is one of the most vital thinkers among his colleagues. Such a one should be left entirely undisturbed. In all cases we are in duty bound to find for each particular labor to be assigned a man who can do it without departing too widely from the things in which he is naturally interested and to which he is temperamentally adapted.

It is essential to our best welfare not only that a correct judgment of values shall be maintained among practised scientific workers but also that the younger men as they come along shall be led to a proper emphasis of values. There is a tendency in some quarters to magnify the importance of editorial work and to esteem it as the basis of as high preferment among one's colleagues as almost any service that could be rendered. There is danger that this error shall be particularly insidious in its influence upon the younger scientists; and it needs therefore to be brought into the light of day where it can not live. It must not be true, and we must not allow the feeling to develop, that it is likely to be of personal advantage to our scientists for them to give up a significant portion of their creative labors in order to perform editorial or any other duties of a secondary sort. Creative work alone produces the more fundamental values. It would be injurious to the cause of science in our country if anything should be done to create the feeling that it is a light matter to lay down research work from any considerations whatever having to do with less vital matters. Let us make sure that there is nowhere in our scientific atmosphere a source of infection breeding such dangers to our young scientists.

In certain subjects, as for instance that of pure mathematics, there is at present a great lack of scholarly and sufficiently complete expositions in English of the matters of central impor-

tance. The number of mediocre or very elementary books is great, in fact so great that it seems to be desirable to lessen the future output by means of a definite public opinion disapproving the appearance of books not having a sound scientific reason for their existence. But it is a distinct service of a high order, above that of editorial labors, to produce scholarly expositions from a sufficiently advanced point of view.

It is therefore important that we shall encourage the production of such books. Two essentials will have to be supplied: it requires well-trained scientists of keen insight to prepare the manuscripts; then there must be publishers willing to invest capital in the enterprise in order to put the book before the public. The latter need will take care of itself and writers will be encouraged to supply the former if such books begin to have a sufficient number not only of readers but also of purchasers. We need to develop the disposition to distribute more widely in public libraries the scientific books of higher grade and the inclination on the part of scientists to enlarge their private libraries. The existence of a large number of discriminating buyers would certainly stimulate the production of the books desired. To buy as many important books as you can use with profit and not as few as you can get along with is to do good to yourself and also to render service to the cause of science.

Nearly all scientists in America have superimposed upon their other activity that of the teacher. Unfortunately the time and energy of not a few men of originality and insight are too largely consumed in elementary instruction which might well be given by persons of less training and sometimes with advantage to the learner. Thus we might release the men of other temperament for that different work which can be performed by only a relatively small number of people. But it is desirable, from the point of view both of the learner and of the scientist himself, that lectures on advanced topics shall be given only by men actively and vitally engaged in research. To the former comes the value of contact with insight, power, and enthusiasm; to the latter, the inspiration of seeking a living exposition by means of which a new mind shall acquire a previously unrealized power.

In order that teaching shall proceed to the best advantage there must be some organization by means of which the various parts of the work shall fit together; and this calls for administrative officers. Unfortunately the work of these in American institutions is magnified beyond its deserts, especially in some of the newer universities which only lately have laid aside the garments of the college. In such institutions many hold-overs

from the old régime are to be found, both as heads of departments and as other men of the more advanced rank in the university. Some of these are such that no word of criticism is to be spoken against them; and some are at the other extreme without the grace to remove themselves from an impossible position. These evils do their greatest harm perhaps in operating to make it difficult for the youths to form correct judgments of value.

As science develops it is of increasing importance that we shall prepare histories of it in the form likely to be made useful in the several ways in which they may be of value. There are three distinct primary needs which may be supplied by histories of science, namely: To enrich the general culture and intellectual life of educated people; to enable a scientific worker to quickly orient himself in a chapter of a science so as to proceed most readily to a detailed mastery of its literature; to enable a scientific worker to ascertain with completeness what has already been attained in a given subject.

To serve any one of the three purposes indicated requires a very different treatment from that needed in the case of the others. The literature at present in existence is totally inadequate in each of the classes; and it seems now likely to remain so for some time. For a few well-confined subdivisions of the several sciences, and for such alone, have we sufficiently comprehensive histories for the workers in particular sciences; and nowhere is there yet made suitable provision of historical material in science for the needs of general culture. In this portion of the history of thought, therefore, is to be found a great opportunity for service to the cause of science and of human progress. The work to be done calls for full training and for insight of a high order.

But the primary and most fundamental requisite for general scientific progress is discovery and verification of new truth. After the worker has been found who may enter upon this labor the next essential is his acquisition of knowledge in the domain in which his energies are to be expended. He must be familiar with a certain measure of the results obtained by his predecessors. The only approach from our side to the boundary of knowledge is along a path which leads through fields of truth already explored; and he who is unable or unwilling to travel such a path will waste in fruitless endeavor whatever energy he expends in trying to penetrate into the unknown.

But the path along which he approaches the boundary of knowledge may vary greatly with the individual thinker, being affected both by his temperamental aptitudes and by his total

measure of power. One will see by an almost unerring intuition the significance and bearing of a given restricted body of doctrine as soon as he is in possession of the truth which constitutes it. Another must know in detail the various related doctrines before he can come to a realization of the import of the first one. It is clear that two aptitudes as widely separated as these demand distinctly different means for the acquirement of truth and for the realization of further progress.

Perhaps the matter may be clarified by a figure. The man of less native force will probably find it necessary to approach the boundary of knowledge along a spiral path which starts from some place in the interior and winds round and round repeatedly, always coming a little closer to the boundary and approaching it asymptotically. This method requires the maximum time and energy for reaching the neighborhood of unexplored territory. Moreover, the line of progress is along a path not far removed from a parallel to the boundary. Consequently the component of momentum in a direction perpendicular to the boundary is small, at least in those places where the boundary is more regular. One who proceeds along a spiral path of learning will therefore have the greatest facility for penetrating the unknown if he finds a narrow cape from the continent of ignorance projecting far into the ocean of acquired truth; and here he will carry on explorations of value, but will hardly be able to open a new region of large extent.

It is obvious that discoveries originating in this way tend to round out and make regular the total body of known existent truth. But it is unlikely that they shall bring within our grasp any body of truth of essential novelty. It would seem therefore that the spiral method of approach to the boundary of knowledge is best suited to those prospective researchers who are of mediocre ability; and that others, having first acquired a suitable general fund of knowledge, should then proceed more directly toward territory as yet unexplored.

This calls for what may be termed the radial method of approach to the boundary of knowledge, that in which one proceeds along a path more or less nearly normal to that boundary. We suppose that one who is to go in this direction shall have first acquired a suitable preliminary acquaintance with the more fundamental matters in his particular science as a whole. Moreover he must be one of those of greater native force or he will be lost in proceeding directly far into unfamiliar territory. But if he has the proper central fund of information and the requisite native force of intellect he can proceed along such a radial path with increasing momentum. Now, practically the

whole of this momentum is in a line perpendicular to the boundary and consequently will serve as a means of breaking through and perhaps of penetrating far into previously unexplored territory.

It is obvious that discoveries arising in this way have the greatest likelihood of opening up new regions of vast extent; and they may even issue in the introduction of fundamentally new ideas. Perhaps our greatest progress is realized from an assault of this sort upon the unknown, one along a fortunate direction chosen by dumb intuition or by a prevision of the character of the advance to be anticipated.

The advantages of radial approach are not altogether denied to those who are restricted by inherent limitations to the spiral method of acquiring known truth. After they have come into possession of a certain general body of doctrine they may select out one line of development in it and proceed through this in a radial direction to the boundary of knowledge and thus have the advantage of a realization of the full momentum of their progress. But they will lack the zest and enthusiasm which characterizes a first view of beautiful truth, and this will usually dim their insight and preclude the greatest conquests. Nevertheless, it must be admitted that some of our very important discoveries are made in this way, particularly among those which are intimately associated with bodies of doctrine for a long time in a state of high development.

In estimating the measure of excellence in scientific activity one can not leave out any fundamental element in the culture of mankind. Our development is as a whole and not in fragments. To be sure, the growth at any one moment may be in spots; but there is a constant interaction among the various parts and a consistent evolution of the whole. One portion of the body of truth can not develop far while all other portions are in a resting state. Each needs the stimulation which comes to it from discoveries beyond its own boundaries and the encouragement consequent on seeing the fruit which itself yields in other domains. Let us therefore consider the values arising from the interaction of science with other general divisions of thought.

Among certain scientists there is a tendency to underestimate the value of philosophical thought owing to the influence of a subtle error in logic arising from reaching a conclusion without formulating wholly in consciousness the argument by means of which the conclusion is attained. Among these scientists it is the custom to reckon as a part of science any well-established results which have met what seem to be the appro-

priate tests of truth, regardless of how these results are first obtained. Therefore, whenever any range of thought has reached a stage of development at which it is able to exhibit definite conquests it becomes, at least in the region of these conquests, a part of the general body of science.

Now in philosophy there has always been much that is speculative. But if philosophy should get its feet on solid earth and attain a conquest definitely achieved these same scientists would take the achievement out of the realm of philosophy—and perhaps rightly so—and annex it to the domain of science. Such a procedure seems rather hard on philosophy; but it might be passed over in silence if it were not followed by an injustice which it is made to support.

These same scientists when they have taken out of philosophy every definite conquest and transferred it to the realm of science, then intimate that philosophy is without essential value; and to drive this intimation home with compelling power they demand with gusto to know what is the single definite achievement belonging to the domain of philosophy, whereas, according to their accepted principles, any definite conquests made by philosophy would pass from philosophy into science in the act of becoming definite. Thus they draw the line of demarcation between science and philosophy so that all definite conquests lie in the domain of the former and so that only speculative or unestablished opinions belong to the latter, and then they decry philosophy as useless because it contains no body of well-established truth as part of its permanent possession. This procedure is unscientific and unjust.

In order that philosophy, even so restricted, may be denied a place of importance in the development of scientific truth, one must show not only that it has made no permanent conquests but also that it has failed to assist materially in preparing the way for the advancement of science. This latter can hardly be maintained even by the most narrow devotees of science; for it is notorious that the atomic theory and the theory of evolution, central as they are in all modern natural science, were at home in philosophy for generations before they took a form in which they might be used in the domain of more exact truth. Their presence for a long time to the thought of mankind was one of the essential prerequisites to their use in connection with knowledge obtained by experiment and observation. Thus two of the most fundamental conceptions in modern science, one having to do with non-living and the other with living objects, have come into their own with the assistance of speculative thought.

From many points of view we are forced to a consideration

of the question as to where philosophy ends and science begins or to the conclusion that there is no dividing line and that the two names represent different aspects of the same thing. Now it seems clear that there is a difference in character between the extremer speculations of philosophy and those parts of science which are so far advanced as to furnish means of precise and accurate prediction. And if there is this essential difference in the more widely separated parts of the two bodies of thought it appears that we must grant them to be different even though at some places they may approach so closely together as to have a common boundary.

Assuming, then, that there is a valid distinction, let us raise the question in some such concrete form as the following. The early statements of the atomic theory of matter (among the Greeks) are clearly speculative in the highest degree and as such belong to philosophy; in our day the atomic theory is characteristically scientific, having the most intimate connection with a large body of experimental results. In the development of this theory where does philosophy end and science begin? The question has a two-fold aspect. Historically, when did the exclusively speculative character of the doctrine take on a scientific form? and to what extent had the theory advanced historically when one should first begin to call it scientific? Logically, what was the state of affairs when one may properly begin to speak of the theory as scientific?

To follow out these questions with care would probably throw considerable light on the fundamental problem as to the relation between science and philosophy. One ought if possible to establish certain definite criteria or sets of criteria of such sort as to distinguish with clarity between that which is scientific and that which is philosophical, except possibly for a narrow ambiguous band lying along the common boundary. Thus, if a theory has reached a stage where it enables us to predict with accuracy certain definite phenomena, there seems to be no doubt that it should be classed as scientific. If this quality is absent should we say that the theory is still in the less advanced stage of philosophy? Or, should we make our test for scientific character less stringent and say that the theory, whether established or not, belongs to science if it is so formulated as to enable us to conceive definite means of subjecting it to experimental test? These are questions on which as yet there seems to be no general agreement.

In science itself there are two bodies of doctrine so distinct in some of their leading characteristics as to be often opposed one to another, namely, natural science and mathematics. In

fact, one often has in mind the former when he speaks in a general way of science, using no qualifying word. In such wise have we used the term in the foregoing study of the relation of science and philosophy. Let us now still more definitely make the separation and proceed to a consideration of the relations between natural science and mathematics.

Some workers seem to resent the interference of mathematics with their comfort in the conclusions of descriptive science and its demands that observation shall be reduced to measurable elements and the laws of nature be expressed in mathematical formulas; other thinkers believe that natural science is real science only in so far as it is mathematical, that it is only through mathematics that true science can be understood, and that without mathematics no science can develop to maturity. One delights in observation and the record of facts, believing that he has understood a class of phenomena when he has given a general description of their relations, order and connections; the other considers the mathematical formula as "the point through which all the light gained by science must pass in order to be of use in practise."

It is clear that observation alone is insufficient for the understanding of a physical fact. Liebig has formulated three conditions which he considers necessary:

> We must first study and know the phenomenon itself, from all sides; we must then determine in what relation it stands to other natural phenomena; and lastly, when we have ascertained all these relations, we have to solve the problem of measuring these relations and the laws of mutual dependence—that is, of expressing them in numbers.

On this view we should have, apart from measurement and hence apart from mathematics, no science except in the preliminary stage preceding the considerable development of any chapter. Some have gone further and have maintained that there is no science whatever except that which stands definitely and clearly on measurement, that the essence of science in the whole and in every part is measurement and solely this.

But this extreme view is not upheld by the position of modern science. There is a significant chapter of chemistry, for instance, in which precise results of a definite character are obtained under the guidance of a theory in which something essentially different from measurement holds the central place, namely, the linking and spatial arrangement of atoms in the molecule. There is a difference of properties accompanying identity of molecular formulæ where we are forced to admit even the same atomic linking and can ascribe the existing difference only to unlike positions of atoms in the molecule. This

is the central notion in the chapter. It has been formulated so definitely and the facts of experiment have been associated with it so closely that there can be no doubt of its correspondence with some esential reality as the main feature involved. It is obviously not in the same class of ideas with measurement.

This leaves unanswered still the question as to whether a self-contained body of well-developed scientific truth can be isolated which has been established and may be expounded without essential use of the method of measurement. It will readily be admitted that some of the preliminary work in the creation of a science consists in the description of phenomena and their analysis as to qualitative aspects. But it seems that we can not bring any chapter of science to a resting state of relative completeness until we have laid the basis for it deep on a foundation of accurate measurement. In the example cited from stereochemistry, for instance, we have in the advanced stages a portion of theory depending primarily on considerations of relative positions in space; but antecedent to the development of this portion of the theory and necessary to its creation and even to an understanding of it is a large body of doctrine resting on detailed measurement.

It appears, therefore, that no science can come to a relatively high state of development until it has been made to rest on a mathematical foundation. Those who insist most strongly on the kinetic or mechanical view of nature appear to believe that physics and chemistry, the basic sciences of natural phenomena, "are destined to become ultimately merely chapters in dynamics as the doctrine of mechanical motion." But probably Maxwell is nearer the heart of the matter when he insists that a physical theory should be at the same time both mathematical and physical.

It seems that no body of thought has at the same time been of more importance in human progress and been criticized more freely than the science of mathematics. Much of this criticism appears to be good-natured and to amount to but little more than a quasi-humorous way of expressing the critic's own unashamed ignorance. At first sight one might treat this as harmless; but from the point of view of the general interest it can hardly be passed over in such a way. How this ignorance is to be overcome I can not say. Perhaps one of the first requisites is to find some means of overcoming the shamelessness with which individuals otherwise well trained contemplate their own ignorance of mathematics.

The mathematician himself is not disturbed so far as the welfare of his own science is concerned; but it is sometimes a

matter of pain to see the general loss which arises from such ignorance and also from the severer strictures of the more pronounced adversaries of mathematics. In no other case, however, have the criticisms been so severe as those meted out to the infinitesimal calculus in the infancy of its development; and never have the fondest hopes of the founders of a science been so far surpassed by its actual achievements as here where the subject has become central in practically every field of pure and applied mathematics.

It will be instructive to examine briefly the criticisms thus met so early by the infinitesimal calculus. Some persons attacked the certainty of its principles, attempting to show that its conclusions were at variance with those obtained by methods previously known and accepted as sound. Some who labored primarily with matters of morality and religion attacked the new departure of thought on general grounds; they repulsed themselves by unwittingly displaying their ignorance of the thing which they criticized. One man, who entrenched himself in masses of calculation, pronounced the procedure of the new calculus unsatisfactory because of the indeterminacy of the form in which certain results appeared; but he afterwards acknowledged his error and admitted that he had been urged forward by malevolent persons—a thing (let us believe) which does not often happen among workers in science. Christiaan Huygens, whose opinion probably carried more weight than that of any other scientific man in his day, believed that the employment of differentials was unnecessary and declared that Leibnitz's second differential was meaningless. But these and many other criticisms never hindered the development of the new calculus, but served rather to aid in clearing off certain excrescences which had nothing to do with its essential characteristics and in helping it to that central place of importance which it holds to-day.

The criticism last mentioned is one which is made so often that it is profitable to dwell longer upon it. So often the mathematician hears: "What is the use of what you are doing?" He knows a thousand answers to this question; and one of the most effective is that which history has given to the criticism of the illustrious Huygens. The recently developed subject of integral equations has sometimes been confronted with the inquiry: Why develop this theory? Will not differential equations serve the purpose? But the mathematician goes calmly ahead with the development of those things which interest him, just as he did formerly; and in the new case he anticipates with confi-

dence the same triumphant justification in the event which has uniformly crowned his labors in the past.

The natural sciences have also been subjected to the same criticism. A narrow view of practical usefulness seems always to dim the vision of those who are unlearned in the progress of scientific discovery and the means which have released into activity the powers of man. They judge from the standards of a daily life in which the prime energy is constantly given to procuring food and shelter or to laying up a store for the future; and it is hard to see the values in a work which does not contribute directly and immediately to such ends. They are not to be judged harshly for this error. It must be admitted that the experience of most of their ancestors leads in the direction of this false judgment on their part. A small number only of the men of each generation see clearly the remoter concerns of mankind; and even they have come to this vision partly from the spur of astonishment at the wonderful values evolving from time to time from abstract considerations.

One of the most remarkable and immediate of these unpredicted values, and at the same time one of the more modern, is afforded by the germ theory of disease. In the middle of the nineteenth century bacteria were known only to a few experts and in a few forms as curiosities sometimes appearing in the field of vision under the microscope; and no one dreamed of their being of importance to man. In fact those who studied them were thought to be wasting useful energy and were subjected to ridicule. But in the midst of wild conjectures and worse logic a nucleus of facts was isolated and some understanding of them began to be realized. In 1860 putrefaction in wounds was ascribed to them, and a few years later their relation to certain diseases began to be known. Then a knowledge of their life-history and action developed rapidly. To-day we understand not only how they are of great concern to us in disease, but also how their action in other ways is of profound importance to our welfare. They are so essential to agriculture, for instance, that our race could not long preserve itself in the present numbers and state upon the earth if the soil were not constantly renewed through the activity of certain of these minute organisms.

Throughout its whole range, from the most abstract considerations in mathematics to the concrete descriptive developments of bacteriology, science at some stage must face the insistent question of its usefulness; and this question it is always answering in ways astonishing even to its ardent supporters.

In the development of physics the infinitesimal calculus has

persistently played a leading rôle; its interaction with experimental results has been and is fundamental and necessary to the progress we have witnessed and yet see to-day. From this creation of the mathematicians and the use made of it by the physicists the world has received a good practically immeasurable in its extent. Sometimes we are tempted to assess the advantages due to each of these elements; but one can hardly expect success from such a venture. Logically the mathematics is prior; for it could exist of itself, while the physics probably could not. But psychologically and practically they are so bound up that no separation can be made. Were the mathematics swept away, much of physical theory would likewise have to go; but on the other hand much of the mathematics would never have existed had it not been called into being by the demands of physical science.

This affords us a beautiful and instructive figure of the interaction of one body of truth with another. Neither of these two would exist in their modern state without the spur which it has received from the other. Neither of them should glow with a spirit of boasting. A humble acknowledgment of fundamental indebtedness is more fitting; and it is pleasant to see each characterized by a fervent zeal to return good for good in full measure.

Before turning aside from the relations between natural science and mathematics let us consider briefly a conception or expectation which has arisen in some quarters and having to do with a more fundamental and far-reaching use of mathematics than any yet made. It is connected with the fact that every branch of physics gives rise to an application of mathematics and the consequent feeling that there must be a deep underlying reason for this and a subsequent close relation of phenomena which probably makes them capable of an explanation from a single point of view consistently maintained.

If there is a "hypothetical substructure of the universe, uniform under all the diverse phenomena," it would appear that there must be some means of ascertaining what it is and of giving to it a mathematical expression and body. At any rate the expectation of such a thing has arisen; let us hope that the event will show that the anticipation is well grounded in the nature of things.

It appears that the earliest contributions to just such a development are already in existence; that the now current theoretical accounts of radiation, diffusion, capillary action and molecular behavior in general have just such characteristics as one would expect to find in the early stages of a mathematical theory of the substructure of the universe.

At the opposite pole of knowledge is another stream of thought whose existence or influence is mainly left out of account by scientists just because it is so far removed from that with which they are accustomed to work. But it has profound influence upon life and even affects science in some measure directly though remotely. This is the unsystematic thought which finds expression in literature and art and sometimes in unorganized philosophic speculation. For the most part the domains of systematic and unsystematic thought have little effect the one upon the other; and yet it must be apparent to one who analyzes the fundamental characteristics of our age that these two great arteries of our culture must have somewhere a vital connection. Only rarely does an important truth find its first expression in unsystematic thought and later take up a place in science; and when it does so it is much changed in the process. The poetry of science has not been written and its cultural elements have not found their way into the general thought of mankind.

And yet all science must receive its initial spring from ideas no more definitely organized than the vague intuitions which strive only half successfully for expression in the form of literature or art. Between the two there is, however, this fundamental difference. Even in its early stages a branch of science has to do only with a restricted range of phenomena. It is necessary to study small sets of closely related matters if we are to get that detailed information on which alone science can rest. We can not obtain definite and precise scientific information about the whole environment at once. We must take it by piecemeal; and so far we have succeeded only where we have isolated the phenomena into distinct and sharply-defined groups. It is quite otherwise in literature, for instance. Here the whole stream of life is contemplated at once, in its fullness and complexity. The results obtained are less definite and are more subject to change from age to age than those of science. Moreover there is less uniformity in the way in which they affect the individuals who consider them. But they have a breadth of reach over the forces of life and the motives of conduct which can be realized only by a single grasp of the whole complex process. Each of these two realms of thought, in its central characteristics, has lessons of value for the other.

Between these two extremes is a middle ground lying undeveloped. One can hardly feel that it is covered by the generalizations of philosophy. To synthesize the results from the separated domains is hardly enough. One desires a means of grasping a more complex portion of reality, vibrant with the

pulse of life, and of reducing it to definite order. It appears that no known methods are sufficient for dealing with this problem. Shall we conclude that it is impossible of solution or merely that we have need of a further development before we can expect success with it? Thought as embodied in literature and art took a wonderful form in ancient Greece. There science had merely a beginning and did not acquire sufficient strength to live vigorously through the middle ages. Many centuries were still necessary for a development of its method. Will another period of like extent lead us to other methods equally novel and suitable for the development of this middle territory between literature and our present science?

The logical evolution of science from the vague to the definite is instructive from many points of view. Through untold periods of time for many ages the human mind moved very slowly toward that isolation of phenomena in thought which is essential to the creation of exact science. In action such isolation can not be effected; we must take the whole complex environment at once and do the best we can. This characteristic of the practical life has doubtless been one of the chief factors in delaying the abstractions now so uniformly present in scientific investigation.

In the construction of each particular science we pass through a sequence of stages similar to those which have characterized our general progress in the development of science as a whole. First of all, there is a period in which a given class of phenomena affect us vaguely and we are only half-conscious of their presence as a connected entity in the complex of the environment. They continue to affect us and we have a growing sense of related experiences proceeding from a common source. This arrests attention and excites interest. From being merely passive recipients of impressions we now come to be active in our relation to the phenomena; we observe them directly, and we seek means to affect them so as to ascertain their course or character with more accuracy. We pass to the stage of vague and gross qualitative description of the phenomena. In the course of our study this gives way to a qualitative analysis of the phenomena and a study of their qualitative relations of coexistence. Such is the state to which the biological sciences of the present day have attained. Sociology finds its place even further back in the scale.

Qualitative considerations never become definite enough to meet the ideals of science. It is necessary to go further and find out the quantitative aspects of phenomena and to determine the exact relations of their coexistence and succession; in

a word, to render the account mathematical. Once Newton and his followers had succeeded in deducing from a few laws, easily stated and comprehended, the essential facts of astronomical motion as obtained by direct observation, it was impossible that any science should rest comfortably in a state in which qualitative considerations are dominant. The astronomical view was certain to spread into molar and molecular physics and even to find a place in the biological sciences.

The various divisions of physics proper were the first portions of other sciences to approach the ideal conceived on account of the success in astronomy; and chemistry has followed at a distance. As late as 1873 it could be said:

> No theory has as yet been formed in chemistry which, starting from a definite principle, attempts to deduce results of experience as necessary consequences.

Measured by the standards attained by Maxwell and his followers in the theory of electricity, chemistry is still seen to be far short of that standard of excellence which we are able to conceive definitely, notwithstanding the remarkable developments which a generation has witnessed. Perhaps it will not find its true place until we are in possession of more effective means for getting into the atom—not merely the molecule—and studying the properties and connections and positions of its parts.

When a science has reached a certain stage of development, varying greatly with the character of its material, it begins to throw off into the body of society great practical or even esthetic values which could not be realized without it. Astronomy has enabled us to have some conception of the vastness of space and the hugeness of the mass of matter, perhaps infinite in its totality, distributed through this space. Geology has released the imagination to contemplate the enormous periods of time and through its influence on biology has rendered marked service in making possible our conception of the long progress of life on the planet, culminating in man. Mathematics, by exhibiting a body of truth which can live through millenniums without needed corrections and at the same time can grow in magnitude and range and interest, has given the human spirit new ground for believing in itself and for rejoicing in its proved power of consistent thought.

In addition to values of this more ideal sort there are others of a practical nature. Two of the most remarkable are those afforded by chemistry and the theory of electricity. The latter has enabled us to transmit readily and over great distances immense stores of energy, either from a central plant or from

some permanent source in nature, and to utilize it for a great variety of purposes. The former has furnished the necessary means for founding certain industries and has contributed essential elements to the maintenance of many others.

But the excellence of science is not altogether in itself or in its fruits. It has interactions with all the fundamentals of life and thought. It moves upon itself, part upon part, and so brings out many values. Of especial import is the effect within itself of the influence exerted by mathematics and natural science, the one upon the other. But it reaches out also beyond its own domain into all the activities of man, both practical and theoretical and speculative. It so moves in his philosophy that thinkers not a few now maintain that the present duty and function of philosophy is to think science, to organize its results and to scrutinize their validity. It so affects daily life in many ways that it can not fail to exert a profound influence upon the unsystematic thought of literature and art as they strive to give expression to the fundamental spirit of life in our age.

From a certain point of view these four main divisions of Thought—mathematics, natural science, philosophy, that unnamed one ruling without definite system in the domain of art and literature—are the stones and brick and mortar from which is builded the culture of the time, into which are wrought the values received from the past, and through which our development shall proceed to the acquisition of new power for further conquest. We break the environment into parts in thought and from these we fashion new objects such as never before existed in the universe—objects both concrete and ideal—and these we put together in ways well-pleasing to ourselves to serve the ends we propose or erect the constructs we conceive.

But this is too mechanical to be the whole truth. The more profound values lie deeper and have their fruition only in the fullness of the character of man. If science did not touch a more profound matter than mere motion or reach to constructs which can not be adequately pictured by material symbols, it would fall far short of the glory of Living Thought. But it does react in a profound way with all our activities. In fact, the elements of all Thought are parts of one body, living and organized, inspired by the breath of the Universe itself and pulsating with the life of truth in its deeper manifestations.

THE RELATION OF PHILOSOPHY AND THE SCIENCES

By Professor G. R. WELLS

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REMARKS on the relation of philosophy and the sciences are, perhaps, best made by one who is quite often supposed to belong to neither of these groups, namely, a psychologist. For as the philosopher usually views a psychologist as one just outside of the sacred pale, so do the scientists often refuse to recognize him as one of themselves. While I would not be understood as calmly accepting the status of a missing link, I do feel that there is some reason for a certain amount of confidence that the psychologist is able to view the two fields of philosophy on the one hand, and science in general, on the other, with a little more understanding than either is able to exercise toward the other. The reason for this is simply that, whether rightly or wrongly, the average psychologist is forced or persuaded to include in his graduate curriculum rather more of philosophy than any other scientist in training.

Etymologically the term philosophy indicates an interest in all knowledge. (So does the term science, for that matter.) To begin with, no doubt, the philosopher was simply the scholar, the learned man. Aristotle was interested in practically everything. And men very much later than Aristotle have taken or have attempted to take, the whole of human experience for their field. One thinks of Herbert Spencer, perhaps of Hegel, and some lesser men in this connection. But except in the cases of the contributions of a very few gigantic men such spreading out of interest has resulted in a thinning of the product so as to make it all but valueless. I have no intention of applying this judgment to the product of philosophy in general. But I do think that the escape from such a situation is often somewhat narrow, and that there is escape at all is due to the fact that philosophy has for many days practically abandoned all attempt to be interested in knowledge in general. No one would dream of going to philosophy to-day for answers to questions of any kind in science, or indeed, to obtain detailed information of any kind, except, perhaps, in the fields of historic folk ethic, or in the history of thought. It is possibly a little

hard to realize that the day is not so very long past in which the seeker for knowledge in any field of learning would go, as a matter of course, to the philosopher. In other words the philosopher has comparatively lately ceased to be a treasury of all learning.

It is of passing interest that many of the earliest philosophers were chiefly interested in physics. I refer, of course, to the Greek atomists. Some interest in astronomy was evident at about the same time, and also in physiology of an exceedingly rude type. These interests were treated in their relation to each other, but chiefly in their relation or supposed relation to theology. Specialization was not thought of. But the history of thought shows us that from time to time men whose interest was centered, rather than scattered, split off from the main group, no longer called themselves philosophers and abandoned the philosophical method. The influence of the church was exerted to its utmost to retard this process, being interested as it was not at all in the subject, but only in their real or assumed relation to religion, or rather, to theology. And for this and other reasons, but chiefly for this, it is really late in the history of civilization before physics, chemistry, astronomy, geology and biology were able to constitute themselves as independent lines of human activity, to pursue investigations along lines suggested by their several special interests, and to build up their own bodies of knowledge by using all the ways and means that their initiative and individuality suggested, or at least all that church and government regulation allowed them to use. But such separation always resulted in lasting benefit to all concerned, except possibly certain priest-philosophers, whose individual opinions may have been outraged.

The last of the children of philosophy to leave the parental home is psychology. In fact she is at present just in the act of setting up an establishment of her own, and still remains very much under the influence of her parent. She might be said to be a spoiled child, and to have been too long content to have her affairs managed by her parent. But the signs of the times are indubitably to the effect that psychology *has* set up her own house, and intends to administer it herself.

Now that psychology is leaving or has left the philosophical rooftree I think it may be admitted that to philosophy the house is left somewhat desolate. Perhaps that is the reason why there has been some persistent objection on the part of the parent to the daughter's leaving. But, however that may be,

the situation is such that the question becomes pertinent, "What is the proper field of philosophy?" While that question is pertinent my attempting to answer it would be impertinent, and I do not intend to so attempt. I have not the necessary training, whatever that may be, nor is it my place, nor is it my interest to answer it. But answered it must be, by some one, before very long. A smaller question is included in the larger, that of the relation of philosophy to the sciences which it has engendered. It is to that question that my remarks here are directed, but even it I do not expect to answer in full.

To begin with, let me emphasize the fact that the various special sciences which have left the philosophical fold, have not thereby ceased philosophizing. There has been much very significant philosophizing done within the various scientific fields and most of it has been done by men who would be apt to object if they were to be termed philosophers. They would insist that they had not philosophized. But like Monsieur Jourdain and his prose these men have been talking philosophy without knowing it. For instance, in biology the mechanistic vitalistic controversy is in its very nature intensely philosophic, but the worth-while contributions to the discussion are, in an overwhelming majority of cases, made by men not ordinarily listed as philosophers. There are exceptions, some valuable contributions have been made by soi-disant philosophers, but these are true exceptions. I can fancy that many men occupying teaching positions in philosophy have wished that they could take a forceful, not to say creditable, part in this discussion. But, alas, it is necessary if one would enter such a controversy that one possess a good deal of technical knowledge of the facts involved, rather more than is in the possession of many philosophers. In physics or physical chemistry the question as to the nature of the atom or that concerning the nature of ether may be properly referred to as philosophical. But few self-styled philosophers have any remarks whatsoever to make on these subjects. In general, it must be recognized that the division between philosophy and science is not a line between those who speculate, who are interested in abstract questions, and those who are not. The difference is of another order.

As far as the facts in the present situation in the world of thought go, the salient difference is in the breadth of interest. The scientist is one whose interests are strictly limited, the philosopher hates to recognize any limits to his field. The scientist is ambitious to go very deep in a narrow shaft, the philosopher spreads his efforts widely. Each attitude has its perfectly obvious dangers.

These two attitudes are so very different that each of them is almost, if not quite, unintelligible to the other. Few scientists are apt to quite understand just what the philosopher is trying to get at, few philosophers really appreciate what the scientist is trying to do. This mutual misunderstanding of the purpose and value of each other's work is only on the surface, and is only a significant symptom of some much deeper differentiation. For below that matter of the breadth of interest there is a deeper and more vital difference between philosophy and science which I wish to suggest in this paper.

I think there is small doubt but that there are really two very different functions of intelligence, two very different kinds of men engaged in intellectual work.

Men who are interested in knowing are either more interested in discovery, in fact, finding, than they are in systematizing their discoveries, *or* they are more interested in systematizing the facts that have been brought to their attention in whatever way than they are in fact, finding. It is a matter accessible to observation that few men are equally interested in both attitudes. If an ordinary man attempts to do both these kinds of work he will seldom be equally successful in both fields. He may excel in one, and be woefully weak in the other. I hazard the opinion that if a man excel in one of these attitudes, in the vast majority of cases he will be very far from excelling in the other. Only mediocre men, or, on the other hand, only very great men are able to work equally well in both. Of course it should be said in passing that this division of the activities of intellectual men is by no means an original observation on my part. Herbart, among others, has made the same statement, using other terms than those in which I have chosen to describe the two attitudes.

The discovering or fact-finding attitude is precisely what the name indicates, a passion for ascertaining shapes, sizes and incidence of phenomena. It delights to find variations, to uncover new and hitherto unrecognized data. Its method is particular, minute. Its reward is a large or small heap of things discovered. Men with this attitude are the pioneers, not, be it noted, the map-makers. They first make the trails, sometimes in haphazard, if not in accidental fashion. They are men whose preeminent virtue it is to *see* well, and in some fashion or other to record what they see.

The systematizing attitude is interested less in minute and detailed facts than in their arrangements, their relations, their patterns. To find, not a new fact, but a new law, is their

delight. It is related of Clerk Maxwell, I think, that in boyhood he would say with regard to some natural phenomenon brought to his notice, "What I want is the particular go of it." Clerk Maxwell was probably of more significance as an organizer of facts than as a fact finder.

The economical thing is surely to recognize the difference between the two functions, and not to demand fact-finding from the systematizers, nor theorizing of great acuteness from the average discoverer. And inasmuch as both of these functions are of necessity essential to all sciences, some arrangement should be made, or recognized if already made, giving equal opportunities to the two approaches to truth. Furthermore, equal credit or reward should be apportioned to painstaking work in either field.

It takes a good deal of courage, perhaps amounting to foolhardiness, to mention names in this connection. I am not possessed of sufficient knowledge of science in general to dream of making even approximately complete lists of men in these two fields, but there are certain cases so outstanding that the mention of their names will illustrate, if it does not prove, my point.

A particularly interesting and complete example of the two attitudes is found in the labors of Tycho Brahe and John Kepler. These two great men worked practically as a team. The one, Brahe, was above all an experimenter, an observer. The other, Kepler, not only was not notable as an observer, but in fact was possessed of vision so far below normal acuteness that he was almost useless for astronomical work from the experimental end. But his contributions to astronomical knowledge were the most significant made by any man up to the time of Newton, with the possible exception of Galileo, and have been exceeded by few men of any time. To this work of Kepler's, however, Brahe made absolutely necessary contributions. Without Brahe Kepler could not have worked. Without Kepler Brahe's work would have verged on the valueless. Brahe's theories were almost grotesque, and Kepler could not have observed with great acuteness or accuracy if he had tried. The two made a perfect team, each being the other's complement to a degree seldom equalled in the history of science.

Newton's work was entirely theoretic. He did practically no fact-finding. He was a systematizer par excellence. His mighty contributions to knowledge were all in the realm of theory. He required absolutely that other men furnish him with facts. Men whose names we hardly know, and no doubt

men of whom we have never heard, furnished the material from which he fashioned his laws. And without the results of the labor of these obscure or nameless workers Newton himself would have remained obscure. When as a marvellous boy he worked out the mathematical calculations which were to establish the theory of gravitation, his calculations concerning the curvature of the moon's orbit seemed to work out incorrectly. There was evidently some serious error of misapprehension. He knew it was not in his mathematics. All his work was based upon the accepted belief of the time that a degree on the earth's surface measured sixty miles. It is to me a curious thing, an amazing fact, that under these circumstances he made no attempt as far as I know to substantiate this measurement. Instead he laid his figures aside for sixteen years. Then a lesser man than Newton experimentally determined that this estimate of the length of the degree was incorrect by nearly ten miles. Whereupon Newton returned to the long-neglected and almost forgotten calculations, and found that in the light of this new experimental fact they were completely adequate. And so he gave the world the theory of gravity and inverse squares. As a fact-finder Newton was not eminent, as an arranger and explainer of facts he was unapproachable.

Clerk Maxwell, Willard Gibbs, Arrhenius! Do these names suggest to you facts or theories? The latter only. I think none of these men did any discovering. But they practised wizardry in the realm of law.

There are equally notable examples of men who have discovered much, but have theorized little, or at least only in unsatisfactory terms. Such men are a little harder than the others to name, because history has valued them less than the others, and many of them, no doubt, have been forgotten.

I have already mentioned Brahe, a shining example. In the realm of biology we find Mendel. Perhaps his work is not entirely characteristic, but at least it is true that he hardly recognized the overwhelming value of his observations. Rowland, the physicist, was essentially a fact-finder, Edison and Marconi come in the same group. Add to these a hundred thousand obscure laborers in the laboratory and in nature, essential men doing absolutely essential work if science is to progress.

A few names are great in both lines, Darwin, Helmholtz, Cuvier, Bovari.

These instances, all too few and briefly put, serve perhaps to illustrate the point I wish to make. Not all men are in-

terested in nor *fit for* experimental work, discovery, fact-finding. Not all men are capable of theorizing in permanent terms. Very few men can do both. The theorizers are the philosophers. But why do they not, or can they not, also observe? The reason is perfectly obvious. Philosophy is not a subject. It is a method. The philosopher is not fundamentally different from the scientist because his interests are broader, but because his methods are different. The two methods require very different mental attitudes and aptitudes. I think it can be put in other words by saying that there is no such thing as philosophy *per se*. Philosophy must be philosophy "of" something, of physics, of chemistry, of biology, of religion, of behavior, and so on. And before the philosopher can work there must be something to work upon, material to be provided by the fact-finder, who must go ahead and pile up these facts to be arranged later by other hands than his.

For this reason it is that the contributions of the earlier philosophers is of little value in the light of present-day science. They had so little to work with in the way of definitely ascertained knowledge that the finest intellects and the best will in the world could have accomplished nothing. How could they observe and state the laws underlying the movements of phenomena when they did not know the phenomena? But the more or less complete failure to recognize this elementary fact has wasted much time and energy.

The place of philosophy as a college department it is presumptious for me to discuss. But is it prophesying for me to suggest that each department of science will, in time, do its own instructing in the philosophy of its own subject? Courses may be included in the offerings of each university scientific department on the philosophy of biology, of physics, of chemistry, of psychology. For the attitude of psychology toward philosophy is not different from that of any other of the sciences, except perhaps that the amount of definitely determined facts makes a comparatively small heap, and that there are some peculiarly involved philosophical problems to be handled.[1]

Beyond philosophizing in the various fields there may be a possibility of some superman relating these various philosophies

[1] My friend, Dr. Dunlap, of Johns Hopkins, has suggested to me that something of the same sort may come in time to be true of history. We may come to hold that history in general is too large, or too vague a subject for one group of men to handle. And so we may some day have in each department courses in history, history of physics, of chemistry, of biology, of government, of economics, of art and literature. That would leave untreated only the history of war. And perhaps some day that may be left to the department of archeology to handle.

to each other. For all facts of experience may, after all, be one fact, in some sense which it is impossible, even absurd to dream of now. But the man who can see and state these universal relations must do so on the basis of his knowledge of the detailed technic of all sciences. It will require a super Newton, a super Helmholtz, a greater Aristotle. But for him who can do it that will be the great task.

These remarks are not to the effect that philosophy has no relation to or place in science; quite the contrary. The value of philosophy to science depends upon the degree to which philosophy is in science, rather than above it. Science uses, or should use, philosophy as a tool, a method, for specific and limited purposes. Philosophy is the handmaid and not the mistress of the sciences. Failure to recognize the ancillary function of philosophy, attempts to enthrone it, have caused some harm to science, but much more harm to philosophy itself. To this error we may truthfully attribute the modern and regrettable belittling of philosophy.

WHY DOES OUR PUBLIC FAIL TO SUPPORT RESEARCH?

By Professor T. D. A. COCKERELL

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WE have in our town a useful and patriotic citizen, sometimes suspected of having Irish blood in his veins, who formerly was a member of the State Senate. Many years ago, the president of the State University appealed to the legislature for a small fund in aid of research. Our senator, in the presence of the president and the students, humorously explained on a later occasion why the appeal failed. The president had made his speech to the committee on appropriations, and was listened to with due courtesy. But when he went out, one of the legislators spoke up: "Say, what *is* research?" Another replied, "D——d if *I* know," and a third proposed "Let's lay it upon the table." There it has rested ever since.

We all had our laugh, in which the president himself good-naturedly joined; but the query of the old farmer-legislator, "Say, what *is* research?" has remained as unanswered by the general public as it was in that committee-meeting. The public believes in education, and will support it up to a certain point. It has a good deal of regard for the sports, and is sure that football is one of the major functions of a university. But it is d——d if it really understands research, and unfortunately is likely enough to be damned eventually by that lack of understanding. Research and teaching are the twin functions of the university, but we are told that teaching is the prime necessity of the day, and research can wait. Actually, it does not wait. We do what we can, as best we can, and for the rest depend for our knowledge on the activities of other states and nations. Intellectual parasitism is so easy, in the sciences or the arts. The world does move, so why not ride upon its back?

Yet the other extreme, of complete intellectual independence, would be much worse. We must of necessity be part of the great body of science, but it should be a living part, not some shell-like external secretion. No state, no city, can afford to be left out of this movement; the eventual penalty is inability to even profit by the work of others.

It should be a matter of personal and social pride. It is of incalculable advantage to any state to belong to the Union, but in the present condition of public opinion there are some disadvantages. If Colorado were a nation, she would as a matter of course have strong institutions for scientific research, and would publish the results of investigations made by her citizens. Being only a state, she is cheerfully willing to see these activities center in other parts of the country, and has little idea of squarely shouldering her own burden. One can easily imagine, and can even find in various countries, excessive local pride leading to absurd results. Our extreme modesty (is that the word?) is not wholly bad. Yet the problem of the states is something like the problem which confronts many of our educated young women. They might fly, could they only believe themselves to possess the wings. Long ages in the apterous condition have produced an inhibition scarcely to be overcome. Yet it will be overcome, in both cases, in the fullness of time.

It is unreasonable to expect the demand for intellectual achievement to develop abundantly among the masses. Reforms start with individuals rather than with multitudes. If we are really to progress, the universities and the men in them must show the way. They must press their cause with so much zeal and sincerity as to convince their students, and their students' students. The torch of Agassiz is growing dim, but it has burned brightly through more than one generation. One looks into the faces of nearly two thousand young men and women at assembly, and thinks, if they all *cared* enough, what might they not do? If they cared for their country and their fellows enough to work and think, year after year, how to make America what America once meant to be!

They don't care like that, and we don't. The stream will not rise higher than its source. If we could do no better, this argument would be wasted ink. But as William James used to say, we practically all of us have unrealized reserves of force. We found that out in the war, and should not forget it in peace. The power is there, both material and spiritual, and only the stimulus is lacking.

I have little respect for the methods of the revivalist, and should regard with grave suspicion sudden conversions to the religion of science. One has to grow into it, and up to it. Yet it is absolutely certain that such growth will only occur in adequate quantity under favorable conditions. It is nonsense to say that the scientific man must be a genius, and as such will defy

the very gods to win a footing. He is usually a man (or woman) of rather ordinary ability, somewhat above the average, who will work when suitably fed, housed and clothed. He wishes to be understood and appreciated, and get due credit for what he does. He does not demand riches, but he is jealous of his rights, and has a sense of justice. All of which is to say that he is a normal American citizen, and desires to be taken as such. He appreciates the indescribable romance of science, the triumphs of discovery, the dawn of new ideas; but all this is part of life, and may come to the merchant, the artist or the farmer, if in somewhat different form. Scientific research is not a thing isolated; it is part of the necessary work of the world, and when once that is understood, it will take its place along with our other normal activities.

We of the university do not appreciate this. We hear it said, and repeat the saying, that one who has the true research spirit will make discoveries, if he has to take the night hours and holidays to do it. It is true enough, but suppose we reverse the picture for a moment. Suppose we imagine a university given over to research, but with students as at present. Professors are paid to do research, and called to account for it, but they teach when they can, or feel inclined. Oh well, if a man has the true teaching spirit he will gather a following, and so the youth will be instructed! The history of the world proves that; Jesus Christ, the greatest of all teachers, got no pay but the cross. Admit all this, and it remains evident to the least intelligent that the education of a democracy can not be left to the occasional influences of spirits too insistent to be crushed. Indebted as we are to the inspired, most of our knowledge and training must come through much more prosaic channels.

No one doubts this, in regard to education. Once there used to be a doubt, but it has long since passed away. We have an educational program, which actually suffers from excess of system. We have gone to one extreme with regard to education, to the other with regard to research. We have no research program. The administration receives no reports on research, and asks no questions regarding it.[1] No one on the campus can tell accurately what the research activities of the university are. Is it therefore remarkable that the public does not know, and legislators possibly think of research as a scheme to give lazy professors more time to loaf? It would be horrible to have

[1] Plans for reform in these matters are under consideration in the University of Colorado.

research administered by a stiff and unintelligent machine, but is that any reason why it should be left to shift for itself unaided? The fault, of course, is not peculiar to any one institution or locality. In some degree it even represents an exaggerated virtue, that of recognition of the independence of the investigator. It is not easy to reach optimum conditions or practices, in teaching or research, but without a certain efficiency in both our democracy can not be adequately maintained.

With a definite program, sufficient means for publication, and dignified arrangements for publicity, I believe there would be no great difficulty in obtaining the support of a majority of the citizens. When it becomes apparent that science can create wealth, banish disease, and illumine the mind, it will be regarded as the indispensable friend of mankind. Scientific writings will themselves become a prime factor in the education of citizens, not simply on the campus, but all over the country and at all times. But it is not sufficient to preach the virtues of science in general. People must be shown in detail, in a thousand ways suited to their particular needs and interests. Hence the propaganda, if we may so designate it, must be in the hands of the scientific men themselves. They alone know the facts, and can speak with conviction.

The problem is not one of finding support for a certain number of research workers. If there were no opportunities for research, the majority of potential investigators would turn to something else, and probably attain greater material prosperity. The problem is, to create and maintain a service, without which civilization will stagnate, and eventually decay. Those who have faith in science do not believe such an outcome possible, but look forward to continually accelerated progress, and the removal of many of the greatest evils which distress mankind.

SCIENCE AND THE STATE

By Dr. WILLIAM SALANT

NEW YORK CITY

THE revolution in sentiment which is being wrought by the war is perhaps most strikingly illustrated by the public recognition of the value of scientific investigation. The idea is fast gaining ground that science is indispensable for the preservation of modern society and may be considered one of the most profitable lessons taught by the war.

According to a declaration of the British Labor Party the advancement of science is placed in the forefront of its political program. The necessity of scientific development is emphasized by the incorporation into its political platform, of recommendations for the deliberate organization of research. This attitude of a group of radical labor leaders toward scientific activity might well be regarded as highly significant of the trend of thought at the present day. For never before has any political party embodied in its declaration of principles the advancement of science as national policy.

Evidence that the progress of science is no longer to be left to chance is also furnished by the announcement of the British Ministry of reconstruction after the war.

Its plans which were published about two years ago indicate that definite form has been given to the recognition that science is the corner stone not only of modern warfare but also of the future society which is to follow the establishment of peace. Judging from the number and the organization of the various boards and committees the object is to stimulate scientific research along different lines on an unprecedented scale. The scheme of this highly important body includes among other items in its program the appointment of two research boards on scientific and industrial investigations, five standing committees, seven research committees, four inquiry committees and three provisional organization committees. Official recognition of scientific investigation was not confined, however, to the British Isles. The colonies have also realized the necessity of scientific investigation as was shown by the establishment of a similar organization in Canada at the head of which is the well-known physiologist Professor A. B. McCallum. It is pertinent

to inquire therefore what our federal government is doing in the way of promoting scientific investigation especially of the type which will be needed after the war. As it is necessary to look into the future and make provision for the solution of various problems which will arise when peace is established a review of the scientific activities of the government and the conditions of scientific work may, therefore, be helpful in preparing for the greater task which is to come, for the analysis of conditions may suggest changes that will lead to necessary improvements in an important branch of the service which has within recent years assumed large proportions. It is not generally realized that the federal government has for years employed a number of agencies for carrying on scientific work of every description. This activity of the federal government is very significant, as it indicates recognition by the American public of the need of scientific work for the solution of practical problems. It shows the realization by the average citizen that science is a necessity and is entitled to support by the government. This has for years been strongly reflected in the liberal appropriations made for this purpose by Congress.

According to Van Hise the appropriation in most federal organizations varied in 1911 from $100,000 to $1,000,000. The total amount per annum available for investigation and supplies was more than ten times as much as any single university or institution in the country spent for this purpose. This did not include the appropriation for the Department of Agriculture, which in 1911 spent twenty million dollars, a large part of which went for scientific and extension work. This appropriation for the Department of Agriculture has since been greatly augmented. For a number of years this department has been receiving enormous sums from the treasury for scientific work. Every science was represented. Every branch of chemistry, human and veterinary medicine, botany, zoology, agriculture and economics were included in its official statement. The estimated cost of the various scientific undertakings for 1917 was over four million dollars, nearly all of which was to be devoted to the various sciences as related to agriculture in all its ramifications. Examination of the program of work published by Department of Agriculture shows that the solution of a very large number of problems have been attempted and still more are contemplated. It may be pointed out that the annual appropriations for research in the various bureaus for 1917 varied between $60,000 and $1,645,640. Moreover the appropriations do not include large amounts spent for routine testing

which has to be carried out in connection with the regulatory work of the department. Very often problems suggest themselves when routine tests are made and demand solution. Appropriations of very considerable size are also made annually for statistical work and for the study of various economic problems. The sum of four million dollars mentioned above does not represent, therefore, the entire amount spent in investigation and research in that department. To this sum must also be added the upkeep of buildings and libraries, and the money invested in the equipment of laboratories. Large appropriations for scientific work in other departments have been made by Congress during the last few years immediately preceding our entry into the war. The Bureau of Standards, the Bureau of Mines, the Bureau of Fisheries, the Naval Observatory, the Geological Survey and other government bureaus have been granted large sums of money for scientific work. It is difficult to quote in figures the exact amounts devoted to research in each case as they very seldom appear in the official records. But the Geological Survey received in 1916 $40,000 for physical and chemical research. Since it is well known to scientific men that research forms part of the activity of these organizations it is reasonable to infer that the total amount of money spent annually for scientific investigation in all the departments of the government must be considerable, though, by no means, so large as that devoted to similar purposes by the Department of Agriculture. The achievement of scientific results is, therefore, a matter of much concern to the public. Their practical importance can not be overestimated since the various undertakings were intended to meet the daily needs of our life. To what extent agriculture, sanitation, preventive medicine and other public activities will be benefited, only those familiar with the problems in the numerous laboratories can fully realize. The possibilities inherent in the opportunities offered for making contributions of great value to the stock of useful knowledge are practically unlimited. It is realized, however, by those conversant with the actual state of affairs that advantage is not taken of the opportunities to the extent that it should, as science neither pure nor applied derived the benefit which might be expected.

It has indeed been pointed out several years ago by Van Hise that the advancement of science in its broader aspects is contributed to by only a few of the scientific bureaus at Washington. The Geological Survey, he remarked, was for a number of years the center of the world for the "advancement of the

science of geology but it is not contributing in any large degree to the advancement of science at present." Such a statement coming as it does from an eminent geologist who had for years been associated with Geological Survey is very significant, as it shows a state of affairs which should not exist. Unfortunately his statement still applies to some bureaus at present. The wide recognition by the public that scientific investigation is a necessity and that the application of the results of research are essential to the very existence of a great nation make it apparent that the conditions surrounding scientific investigation and research in the federal government should not be a matter of indifference to the public and should form an object of especial interest to all scientific workers in the country. This claim upon their attention applies with even greater force to men of science at this hour, for after our entry into the war the number of scientific projects undertaken by the different government agencies markedly increased. Besides, there is every indication that scientific activity, carried on under the auspices of the federal government, is destined to assume much larger proportions during the period of reconstruction than before the war. The number of scientific workers in the employ of the government promises, therefore, to become permanently large and the problems they may be called upon to solve are likely to be more serious and more numerous in the future than they have been in the past. The treatment of scientific workers, particularly those who have demonstrated their ability to carry on and to direct investigation deserves special attention, for upon them largely depend the objects to be achieved. Proper treatment at the hands of bureau chiefs and heads of departments is essential to success in research. The necessity of improving the conditions of scientific work should be recognized, therefore, and the much-needed reforms should be introduced without delay. Initiative should be permitted and encouraged. There is nothing in our form of government to prevent it. The numerous, petty and annoying regulations should be abolished, as they defeat the very object for which laboratories are established. The establishment of an agency to carry into effect a certain project carries with it by implication authority to employ the methods necessary for the achievement of results. It would, indeed, be considered unreasonable to expect results without the necessary physical equipment employed in the experimental sciences. The need of instruments and apparatus is readily admitted and is usually provided. This applies with even greater force to the intangible conditions in the absence of

which scientific work is either wholly impossible or is at best extremely difficult. The need of a free hand to achieve results when any serious project is undertaken is fully recognized and it is, of course, to be assumed that scientific work in the government is and should be made a serious matter. The condition of the heads of laboratories is especially worthy of attention, for upon them rests the responsibility for carrying out the various scientific projects and it is they who are accountable for the results. Practically all of them are near or past middle age and the possibilities of their being called to fill similar positions elsewhere is very remote. If the tenure of office is so uncertain, as shown by a recent case in the Bureau of Chemistry, that their services may be terminated at any moment and on the slightest pretext, directors of laboratories and their associates are wholly at the mercy of their superior officers. Such conditions are absolutely incompatible with the proper performance of duty and the achievement of scientific results and besides compromise the dignity and self-respect of scientific men. Except under very unusual circumstances, as when the head of a laboratory, or a man in charge of a project is not wholly dependent upon his position for a livelihood, the discharge of his official and professional obligations with any degree of satisfaction to himself and in obedience to the interests of his real employer—the public—becomes almost an impossibility. It is not necessary to dwell upon the influence which such conditions will exert upon the morale of the individual concerned and, therefore, upon his scientific work. The result is necessarily and inevitably disastrous. A policy whereby scientific workers of every grade and description would be shown consideration is urgently needed in every bureau and demands immediate attention. Recognition of merit by promoting the industrious and efficient as well as the recognition that a scientific worker, in whatever capacity employed, has rights that a superior officer is bound to respect, under pain of disgrace and dismissal, would keep intact the organization of useful and productive laboratories. This could be accomplished without added expense and would be beneficial to the public service directly by increasing the volume of scientific work and improving its quality, and indirectly by affording thorough scientific experience to young men and women. Increase of appropriations are hardly necessary to carry into action such a policy, but what is more important is the inauguration of a policy looking forward to the transformation of every unit in every bureau into a harmonious effective organization. But to carry out such a policy men of

marked ability and devotion to the cause of science and to the highest interests of the public are needed. The important fact can not be overlooked that one of the lessons of the early days of the present war was that no nation can afford to neglect its scientific workers or to allow its research agencies to be disorganized and wasted. The most patriotic service that can be rendered is to be ready with our laboratories fully equipped and working at their highest possible efficiency. The reconstruction of our national industries to meet the needs of the world's food supplies and the world's industries, as well as to meet the competition of other nations will demand that American men of science be ready as they have never been before to match their research skill against that of the whole world, which necessitates that our government should lead in every branch of scientific work. Its organization of science should be a model to the country and to the whole world, as otherwise we may be outstripped by other nations in industry and in commerce. While private philanthropy has done much for science in this country it should not be forgotten that the government with its unlimited resources can do vastly more than can ever be accomplished by the wealthiest and most generous individuals. Scientific research is a very expensive undertaking. The cost of some of the constituent laboratories of the Rockefeller Institute and of the Carnegie Institution is between forty and fifty thousand dollars a year. The multiplication of such laboratories will undoubtedly become a necessity in the near future. Is it not too much to leave the development of science to chance and to the generous impulses of men of wealth? Besides it is hardly conceivable that private philanthropy will be equal to the occasion in the future for the scientific undertakings promise to be so vast that the government alone will be equal to the task. The movement in this direction is, however, not new for this country. Public opinion will undoubtedly approve of more thorough organization of science by the government and of more liberal support for it in the future than in the past. There is every reason to expect that we can accomplish even more in this direction than the other nations, since our resources are practically unlimited.

INERTIA[1]

By SIR OLIVER J. LODGE

WE are each of us flying through space at nineteen miles a second, probably much more. Nothing is propelling us; we continue to move by our own inertia, simply because there is nothing to stop us. Motion is a fundamental property of matter. No piece of matter is at rest in the ether, the chances are infinite against any piece having the particular velocity zero; every bit is moving steadily at some given speed, unless acted on by unbalanced force. Then it is accelerated—changed either in speed or direction, or both.

As a matter of fact we, like other bodies on the earth, are acted on by two slight unbalanced forces—one which makes us revolve round the earth once a day, like a satellite, the other which makes us revolve round the sun once a year, like a planet or asteroid. Our annual revolution is not because we are attached to the earth; we are not attached, but revolve as independent bodies, and would revolve in just the same time and way if the earth were suddenly obliterated: only then we should find the diurnal revolution transmuted into a twenty-four hour rotation round our own centers of gravity, and the excentricity of our annual orbit very slightly changed. In any case there is no propelling force, only a residual radial force producing curvature of path.

A railway train, or a ship moving steadily, is likewise subject to no resultant force. Propulsion and resistance balance. The whole power of an engine, after the start, is spent in overcoming friction. The motion continues solely by inertia. Any steadily moving body is an example of the first law of motion. You need not try to think of a body under no force at all; you cannot think of such a body on the earth, but you can think of one under no resultant force, *i.e.* under balanced forces. Such a body moves by reason of its inertia alone. It is in equilibrium: it is not at rest.

But we have no sense of straightforward locomotion, and not the slightest clue to either the magnitude or direction of our motion through space. We can ascertain approximately

[1] Amplified from a lecture on "Ether and Matter," given before the Royal Institution of Great Britain, on February 28, 1919.

how the sun is moving with reference to our system or cosmos of stars, but we do not know at what rate that system is itself moving. For all we know it may be moving very fast: hundreds of miles per second.

We have a sense of acceleration however; we experience it in a lift as it begins to descend; and if the sensation is repeated often enough, as on a rough sea, the result is unpleasant. We have also a sense of rotation; we can tell when our vehicle—say a Tube train—turns a corner in the dark. Most animals appear to have a sense of rotation, apparently located in the ear. But we have no sense of direct translation; and we have so far failed to devise any instrumental means for detecting our motion through the ether of space.

The failure is not for lack of trying. Many experiments have been tried, but there is always some compensating effect; so we get no answer to the question—at what rate and in what direction are we moving? The best known experiment is that of Michelson and Morley, the result of which seems to assert that the ether clings to the earth, or that the earth is not moving through any kind of substance. But Fitzeau's classical experiment showed that a transparent body carried with it none of the internal ether of space; and experiments made by myself at Liverpool in the nineties of last century show that a rapidly moving opaque body carries no external ether with it, that there is no perceptible viscous drag or cling between matter and ether, and accordingly demonstrates that stagnation or absence of relative ether drift past the earth is not a reasonable explanation of Michelson's negative result.

The two experiments together, in fact, ought to be taken as establishing the reality of the most interesting of all the compensating effects yet discovered, viz. the FitzGerald-Lorentz contraction of all matter in motion, which the electrical theory of cohesion renders so extremely probable. It only amounts to a 3-inch shrinkage in the whole diameter of the earth in the direction of motion; but it is enough. This slight contraction or change of shape in moving bodies I regard as the definite and interesting compensating effect in this case. Incidentally, moreover, it establishes the electrical, *i.e.* the chemical, nature of cohesion. For given that cohesion is a residual chemical affinity—due to the outstanding attraction of molecules composed of neutral groups of equal opposite electric charges, brought so near together that the attraction between molecules is no longer averaged to zero—then, on orthodox Maxwellian

electric theory, a diminution of this force due to lateral motion is inevitable. And the resulting lateral expansion or longitudinal contraction, or both, is of the right order of magnitude. So this acts as a previously quite unsuspected compensating effect, which exactly neutralizes the drift effect otherwise to be anticipated. Thus, by superposition of two positive consequences of drift, the Michelson experiment, like every other yet made, declines to indicate that there is any drift at all.

Hence, after many such negative results, it seems to become hopeless to enquire experimentally as to our motion through ether. Unless indeed gravitation were exempt from the otherwise universal compensation. In that case the electrical theory of matter applied to the motion of planets might yield a residual result. But my recent enquiry into this problem has suggested that gravitation too is in the conspiracy, and in that case there is some ground for the contention of the extreme Relativists, not only that we do not know our motion—with which everyone agrees—but that we never shall know it: and, in fact, that motion of matter through ether is a phrase without meaning.

I hope we shall not too readily shut the door on further attempts in this direction; and as a conservative physicist I may be allowed to lament the extraordinary complexity introduced into physics and into natural philosophy by the principle of relativity, as so remarkably and powerfully developed by the mathematical genius of Einstein, with complication even of our fundamental ideas of space and time. The complications do not commend themselves to all of us, and I for one should be glad to return to the pristine simplicity of Newtonian dynamics, modified of course by the electrical theory of matter; admitting the FitzGerald-Lorentz contraction, and admitting also the variation of effective inertia with speed. These things do not destroy, but supplement, Newtonian dynamics. They generalize it in a legitimate and intelligible manner. Such complications as these are clearly in accordance with truth and are to be welcomed; but the complicated theory of gravitation created this century by Einstein and developed by his successors, and the consequent overhauling of space and time relations, do not at present commend themselves to me, nor I think to others of what I suppose must be called the older school.

Meanwhile the full-blown theory has the courage of its conviction and has predicted a definite result, viz. the deflexion of a ray of light by the sun's limb, equal to 1.75 seconds of arc. The prediction is going to be tested during the solar eclipse of

May 29, 1919, between Brazil and the Gulf of Guinea. Let the issue be clearly understood. If a star-ray grazing the sun is deflected $\frac{3}{4}$ second it will mean only that light has weight, that the wave-front not only simulates the properties of matter by carrying momentum—as we know it does from the investigations of Nichols and Hull, Poynting and Barlow, and others—but that it is even subject to gravity. For this would be the angle between the asymptotes of a cometary orbit when the comet is moving with the speed of light and passing close to the sun. But the principle of relativity—through the refractive or converging influence of a strong divergent gravitational field—demands a greater deflexion than this, more than twice as great. So there are three alternate deflexions before us, to be settled by observation:

1.75 sec.; 0.75 sec.; and zero.

Let us hope that the result of this or of some other eclipse-opportunity may be definite enough to discriminate clearly and quantitatively between these three alternative values; any one of which should be equally welcome to any lover of truth.

If the first answer is given decisively, it will be a conspicuous triumph for the theory of relativity, and will for a time be hailed as a death-blow to the ether. I claim beforehand that such a contention is illegitimate, that the reality of the ether of space depends on other things, and that the establishment of the principle of relativity leaves it as real as before; though truly it becomes even less accessible, less amenable to experiment, than we might have hoped. Nevertheless the ether is needed for any clear conception of potential energy, for any explanation of elasticity, for any physical idea of the forces which unite and hold together the discrete particles of matter whether by gravitation or cohesion or electric or magnetic attraction, as well as for any reasonable understanding of what is meant by the velocity of light. Let us try to realize the position beforehand; for we shall be handicapped in the progress of our knowledge of the relation between matter and ether until these fundamental things are settled, and until everyone agrees that the ether has a real existence. I want people generally to admit that the ether is itself stationary as regards locomotion, and that it is the seat of all potential energy; and further, at least as a surmise, that it is the medium out of which matter is probably made, and in which matter is perpetually moving by reason of its fundamental property called inertia—a property the full explanation of which must, I expect, ultimately be rele-

gated to and considered as a property derived from the ether itself.

I call this lecture "Ether and Matter," but I might equally well have called it "Inertia," for that is the main theme with which I have to deal—at least in this first part.

Is there anything else, besides matter, which possesses or seems to possess inertia! Faraday discovered that an electric current had a property which bore some analogy to inertia, a property clearly depending on its magnetic field. Every current, even a convection current, is necessarily surrounded by lines of magnetic force, and when the magnetic field is intense the current behaves as if it had considerable inertia. Faraday at first called the effect "the extra current." Maxwell called it "self-induction." The latter is the better name.

To show it I start a current in a circuit containing a stout ring of laterally subdivided iron round which the current-conveying wire is wound, and I put in circuit an instrument which only responds when the current has risen to nearly its full strength. A current usually rises, what is called, instantaneously, but here there is a very noticeable delay between pressing down the key and the response of the instrument. The lag shown is only a second or two, but with care I can adjust it till it is a quarter of a minute. Such delay or lag in establishing a current would be fatal to electric telegraphy. In practice the delay is reduced to a minimum, by using its early values, and the actual response is exceedingly quick. Still, the law of rise of current is quite definite, there is no exception, it is only a question of degree; and the law is the same as that appropriate to the pulling of a barge on a canal. A barge gets up speed slowly, at a rate depending on its mass or inertia, and it ultimately attains a steady speed when the resistance balances the pull.

That is exactly the case of a steady current obeying Ohm's law, the E.M.F. is balanced by the resistance, the propelling force is zero, and the current flows by what we may call its own inertia—its own momentum.

To stop the current you must either increase the resistance or suspend the propelling force. If you interpose an obstacle suddenly, the motion stops with violence—a collision in the case of a train or barge, a flash in the case of electric current. This is what Faraday called "the extra current at break," and if you are holding the wires in your hand when the current is suddenly broken in a circuit of large self-induction you may get a nasty shock.

If you could abolish electric resistance a current would go on for ever without propelling force.

An amazing experiment has been made by Kamerlingh Onnes at Leiden, who first cooled a metal ring down to within four degrees of absolute zero by means of liquid helium, and then started a current through it by a momentary magnetic impulse. Instead of stopping in a minute fraction of a second, as usual, the current went on and on, not for seconds but for days. In four days it had fallen to half strength, and there were traces of it a week later. A most suggestive experiment as to the nature of metallic conduction, as well as a demonstration of the fly-wheel-like momentum of an electric current!

This electromagnetic analogue to mechanical momentum or inertia is explicable (or supposed to be explicable) in terms of the magnetic field surrounding the current, *i.e.*, really (as I think) in terms of a property of the ether of space. It exactly simulates inertia; but is it an imitation or is it the same thing? Can it be said that an electric charge possesses inertia in its own right, and retains it always, as matter does, whether it be moving or whether it be stationary?

The question was brilliantly answered by your Professor of Natural Philosophy, Sir J. J. Thomson, so long ago as 1881. He calculated the inertia or quasi "mass" of an electric charge e, on a sphere of radius a, and showed that it was $m = \frac{2\mu e^2}{3a}$.

The μ need not be attended to now, though it is really the most important of all—being a great etherial constant of utterly unknown value[2]—but for our present purpose the μ merely signifies that the e must be measured in electromagnetic not electrostatic measure, when the formula is interpreted numerically with $\mu = 1$.

At the date 1881 this expression for true electric inertia, though an interesting result, seemed too absurdly small to have any practical significance. Take a sphere like a football, 20 centimeters or 8 inches in diameter; charge it till it is ready to give more than an inch spark, say up to 60,000 volts; then calculate the inertia or equivalent mass corresponding to the charge. If I have done the arithmetic right it comes out one-third of a millionth of a millionth of a milligramme (3×10^{-19}). Absurdly small! Yes, but not zero. And whenever a quantity

[2] I have guessed that it is a density of 10^{12} grammes per c.c. $\div 4\pi$. See "The Ether of Space," Appendix 2; also the *Phil. Mag.* for April, 1907.

is not nothing, there is no telling what importance may not have to be attached to it sooner or later. Nothing real can be so small as to be really negligible in the long run as knowledge progresses. Something at present unforeseen may bring it into prominence. So it has turned out in this case. The infinitesimal result of nearly forty years ago to-day dominates the horizon. It was in some sort the dawn of a new era in physics.

Consider it further. Clearly the inertia depends not on the charge only, but on its concentration. The radius of the sphere occurs in the denominator of the expression. The same charge on a sphere 2 centimeters in diameter would have ten times the inertia; on a sphere as small as an atom the inertia would be a hundred million times bigger still. But then even that is small; moreover an atom could scarcely be expected to hold such a charge. Nevertheless, allowing only a reasonable potential, it might seem that atomic inertia could be sensibly increased by an electric charge. But no, even on a sphere as small as an atom the concentration turns out insufficient; the effect is still excessively minute. Yet as electric inertia at given potential depends on linear dimensions, while material inertia depends on those dimensions cubed, there must be a size when the two are equal, *i.e.* when one might account for the other.

Write the charge in terms of electrostatic potential V

$$e = KaV$$

then

$$m = \frac{2KaV^2}{3c^2}$$

where c is $1/\sqrt{(\mu K)}$, the velocity of light.

Put this expression for m equal to $\frac{4}{3}\pi a^3\rho$, the ordinary mass.

Then the potential at which the two will be equal is

$$V_1 = ac\sqrt{\left(\frac{2\pi\rho}{K}\right)}$$

which, for density of water and for sphere 10^{-13} centimeters radius, is two volts; quite a reasonable electrolytic value, such as is to be expected among atoms.

The moral of this elementary but not very satisfactory argument is that not for bodies of atomic size, but for something 100,000 times smaller in linear dimensions, is it possible to explain inertia electromagnetically. But, forty or even twenty years ago, one would have said—there are no bodies of this size; nothing can be smaller than an atom! The strange thing is that, as nearly everyone knows now, bodies of this size have

been discovered. They were isolated by J. J. Thomson in 1899, having been gradually led up to by Crookes's and many other experiments on cathode rays; and they are shown to be an apparently invisible unit or atom of electricity whose inertia is wholly electric.

The proof of this last statement I can only briefly indicate. It is established by the effect of speed on electric inertia. If an electric charge is moving with something approaching the velocity of light, its inertia increases without limit; and the formula given about 1889 by Heaviside, Thomson, and others, for electric inertia as a function of speed, is, in its very simplest form,

$$m = \frac{2\mu c^2}{3a}\left(1 + \frac{v^2}{2c^2} + \text{higher powers}\right)$$

The velocity of light squared occurs in the denominator, so, before we can observe the increase, enormous speeds are necessary. A cannon-ball, or even the earth in its orbit, is hopelessly slow; and we know no artificial means of getting up such a speed as this last, viz. about 19 miles a second. But fortunately radium does spontaneously what we cannot do, it expels electrons with something less, but not very much less, than the speed of light; and Kauffmann's measure of the mass of these projectiles, thus flying at prodigious velocities, confirms the theory, and removes any doubt as to the reality of purely and wholly electric inertia, for electrons.

Furthermore it was found that the very same electrons can be split off or detached from any or every kind of atom, that there is only one kind of negative electron; and though at first there appeared to be many kinds of positively charged particles, the evidence is tending to the discovery of a single kind of positive electron likewise; so it is natural to suppose that electrons are an essential ingredient in matter. And since they possess inertia, even those which are clearly disembodied electric charges, it becomes possible to surmise that in some sense, or in a certain grouping, they constitute the atom, that they confer upon it the inertia with which we are familiar and that in fact electric inertia is the only inertia that exists.

Electric inertia began as the simulacrum of material inertia, it has shown itself the very same thing, and it seems likely to end by displacing every other kind of inertia altogether.

This is the electrical theory of matter.

Assuming this theory for the present as a working hypothesis, we may say that material inertia is explained electro-

magnetically, *i.e.* is explained in terms of the magnetic field which necessarily surrounds and accompanies every charge in motion; since a charge in motion constitutes a current. For on this view a material body is but an aggregate of such charges grouped according to some definite pattern, positive and negative charges interlaced or somehow intertwined, and so far apart in proportion to their size that they do not interfere with each other or cancel each other, nor apparently overlap or encroach on each other's field, to any measurable extent. Is this possible? It is. For comparing the size of an electron with the size of an atom we perceive that they are relatively of the same order as the size of a planet and the size of a solar system. So it becomes possible to think of an atom as a sort of solar system, with a positive nucleus or sun surrounded by negative electrons revolving in regular orbits round it.

On this view, or indeed in any form of the electrical theory of matter, the atom of matter consists mainly of empty space; in other words, it is excessively porous; just as the solar system is mainly empty space and may be spoken of as excessively porous; the actual material lumps being almost infinitesimal in proportion to the total bulk. A rapid projectile or a ray of light passing through the solar system would be unlikely to hit anything, the chances would be strongly against a collision. So also, if a point be thrown through an atom, the chance of its hitting anything is about 1 in 10,000. It might pass through 10,000 atoms before striking. This experiment has been tried, by C. T. R. Wilson and others, and that is roughly speaking the result. Sooner or later a radium projectile meets with an obstacle and is stopped, but it traverses a good number of atoms on the average; it traverses quite a perceptible distance even in a dense solid, before it strikes a nucleus.

Matter accordingly seems to me—to us I may say, for in this most physicists are I think agreed—a gossamer or milky-way structure, an impalpable accident in the substantial ether. Here a speck and there a speck, but, for the great bulk of it, empty space!

"Impalpable" is not the right word, for matter is essentially palpable. It is because it appeals so directly to our senses that we attend to it so vividly. It forces itself on our attention, while the ether eludes us. And why? Clearly because our bodies are composed—our sense organs are composed—of this very matter. On the material side we are part of, and thoroughly at home in, the material universe. Whereas the ether

is elusive; we know nothing of it directly; and though our eyes are instruments for receiving etherial tremors excited by agitated electrons, we only know that fact—or half know it—by rather recondite inference. Light really tells us nothing about its own nature, but only about the superficial aspect of that gross and palpable matter which has interfered with and scattered it before it enters our eye.

Nevertheless the atoms of this solid-seeming flesh and matter as we know it, when analyzed into constituents, are turning out to be composed each of a definite grouping of ultra-minute particles, the positive and negative electrons, which themselves hardly occupy any space (save as soldiers occupy a country), and which appear to be of two kinds only—the ultimate indivisible units of positive and negative electricity.

THE MECHANISM OF EVOLUTION[1]

IN THE LIGHT OF HEREDITY AND DEVELOPMENT

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V. The Cellular Basis of Ontogeny and Phylogeny

B. Mechanism of Heredity

2. *Share of Cytoplasm in Heredity*

THE evidences outlined in the preceding section demonstrate that the chromosomes contain the factors or genes of Mendelian inheritance, and it has been assumed by many investigators that the cytoplasm of the germ cells serves only as environment or food for the chromosomes and has nothing to do with heredity. Nevertheless there are certain characters of the embryo and adult that are derived directly from the cytoplasm of the egg and since these characters come from the mother and not from the father this may be called "maternal inheritance."

(*a*) *Differentiation of Cytoplasm of Germ Cells.*—The cytoplasm of both male and female sex cells is differentiated. A spermatozoon is perhaps as highly differentiated as any tissue cell; but these differentiations which have been built up during the late stages of spermatogenesis and which serve to bring the spermatozoon into union with the egg, disappear after that union either by the tail of the spermatozoon being left outside of the egg or by its disintegration or de-differentiation after it has entered. At the same time the egg ceases to form yolk while that which has been stored in the egg is gradually used up in the nourishment of the embryo. Consequently since these particular differentiations of the germ cells disappear after the union of egg and sperm it has generally been supposed that all cytoplasmic differentiations of these cells are wiped out at this time, and that the first differentiations of the new individual begin after fertilization in a wholly undifferentiated cytoplasm. However there is positive evidence that this is not the case and that many differentiations of the cytoplasm of the unfertilized egg persist and play an important part in embryonic differentiation (Fig. 22).

[1] William Ellery Hale Lectures before the National Academy of Sciences, Washington, April 16 and 18, 1917.

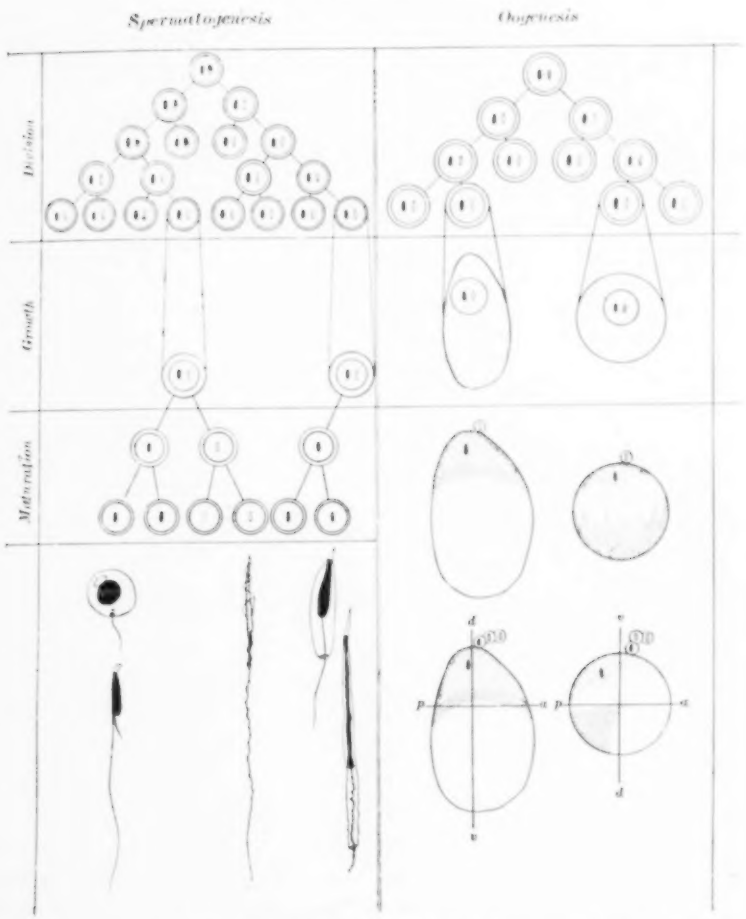


FIG. 22. DIAGRAM OF SPERMATOGENESIS AND OOGENESIS, showing Division, Growth and Maturation Periods and Final Stages of Spermatozoa and Ova. Two chromsomes, one from the father the other from the mother, are shown in each cell before maturation, only one after the reduction division. Three kinds of highly differentiated spermatozoa are shown and two kinds of ova. The differentiations of the spermatozoa are lost after they enter the egg, the polarity and symmetry of the egg persist and determine the orientations of development.

(*b*) *Egg Differentiations which persist in Embryo and Adult.* (1) *Polarity.*—The polarity of the egg invariably determines the polarity of the embryo and adult. In all animals the chief axis of the egg becomes the chief axis of the gastrula, and this becomes the chief axis of the adult in sponges and cœlenterates (protaxonia), or, as in all other metazoa (heteraxonia), this axis is bent on itself by the greater growth of the gastrula on its posterior side so that the chief axis of the adult is a modification of the gastrular axis. In either case the

polarity of the unfertilized egg determines the localization of developmental processes and ultimately the polarity of the developed animal.

(2) *Symmetry.*—In most animals the egg is spherical in shape and appears to be radially symmetrical, nevertheless observation and experiment show that such eggs are sometimes bilateral, as is probably the case in *Amphioxus*, ascidians, fishes and frogs. In the case of the frog's egg it was long believed that the plane of bilateral symmetry was determined wholly and exclusively by the path of the spermatozoon within the egg; more recently it has been shown by Brachet (1911) that primary bilateral symmetry is present before fertilization, though after fertilization the plane of symmetry may be shifted into the path of the spermatozoon. It is probable that all bilateral animals come from eggs which show a similar primary bilaterality and that this differentiation precedes fertilization. In cephalopods and some insects all the axes and poles of the developed animal are already recognizable in the egg before fertilization. Symmetry, therefore, as well as polarity, is derived from the egg and not from the sperm.

(3) *Inverse Symmetry* (*Asymmetry*).—In many animals the right and left sides of the body are not completely alike, and this is especially true of internal organs. This asymmetry is especially well developed in gasteropods in which certain organs of one side of the body are entirely lacking; some species or individuals have these asymmetrical organs on one side, others on the other side, and correspondingly the snail shell coils in a clock-wise direction in one case, an anti-clock-wise direction in the other. It was discovered by Crampton (1894) and Kofoid (1894) that in sinistral species the direction of certain cleavages of the egg (viz., the third to the sixth) was the reverse of the corresponding cleavages in dextral species and Conklin (1903) showed that the first and second cleavages also were in opposite directions in dextral and sinistral snails, and that these reversals of cleavage could be followed cell by cell to the reversal of symmetry in the larva. Consequently the inverse symmetry of these snails may be traced back through the later and earlier cleavage stages to the unsegmented egg itself which is inversely symmetrical in sinistral as compared with dextral forms.

(4) *Types of Egg Organization.*—The polar differentiation of an egg is manifested particularly in the localization of different kinds of materials in different parts of the egg. These materials may be inert pigment or yolk, but their localization

by the activity of the cytoplasm indicates a definite pattern of organization in the cytoplasm. This pattern of egg cytoplasm differs greatly in certain phyla, there being a cœlenterate type, an echinoderm type, a turbellarian-annelid-mollusk type, and a chordate type. The type of egg organization foreshadows the type of adult organization; in ascidians for instance distinct cytoplasmic substances are found in the egg in the same relative positions and proportions as the ectoderm, endoderm, mesoderm, notochord and nervous system of the embryo (Fig. 5, p. 495).

That the fundamental pattern of egg cytoplasm is not influenced by the spermatozoon is proved by the following facts:

(*a*) It exists before fertilization, or it appears so soon after that it could not have been caused by the sperm. (*b*) In heterogeneous fertilization the pattern of the egg is not changed by the foreign sperm. (*c*) Natural or artificial parthenogenesis demonstrates that this pattern exists in the absence of fertilization.

These as well as other facts such as the correspondence between the size of the egg and the size of the embryo (Morgan); the transmission of chromatophores and peculiarities of leaf coloration by the female and not by the male germ-cell in plants (Baur, Shull); the transmission in the egg cytoplasm of fat stains, chemical substances, immunizing bodies and possibly parasites, prove that,

> at the time of fertilization the hereditary potencies of the two germ cells are not equal, all the early development, including the polarity, symmetry, type of cleavage, and the relative positions and proportions of future organs being foreshadowed in the cytoplasm of the egg cell, while only the differentiations of later development are influenced by the sperm. In short, the egg cytoplasm fixes the general type of development and the sperm and egg nuclei control only later differentiations (Conklin, 1908, 1915).

Ontogeny begins with the differentiation of the egg in the ovary and not at the moment of fertilization; at the latter time some of the most general and fundamental differentiations have already occurred. Indeed the cytoplasm of the egg is the more or less differentiated body of the embryo.

(*c*) *Is Inheritance through the Egg Cytoplasm Non-Mendelian?*—Whenever a character is transmitted as such through the egg cytoplasm and not as factors in the chromosomes of egg and sperm it is not inherited in Mendelian fashion. Thus if chromatophores are transmitted from generation to generation in the cytoplasm of the egg and are at no time influenced by the sperm, their mode of inheritance is non-Mendelian. If the

polarity, symmetry and pattern of the egg do not arise during oogenesis, but are carried over unchanged from generation to generation they are also non-Mendelian characters. With regard to the polarity of the egg, it is not certain whether it is transmitted in this manner or not; but its symmetry and pattern of organization are probably developed anew in each generation. However, Kunkel (1903) and Lang (1904) found that inverse symmetry is not inherited in Mendelian fashion and that it is doubtful whether it is inherited at all.

Most of the cytoplasmic differentiations of the egg and sperm arise during the genesis of those cells, just as in the case of tissue cells. Nerve cells and muscle cells differentiate under the influence of maternal and paternal chromosomes, and undoubtedly the same is true of most of the differentiations of egg and sperm; but while some of these egg differentiations persist in the new individual those of the sperm do not. Consequently, in each generation the egg contributes more than the sperm to ontogeny. There is cytoplasmic inheritance through the female only, but some of these cytoplasmic characters may be of biparental origin. If these characters are determined by genes in the chromosomes of cells from which the egg develops this is Mendelian inheritance or "preinheritance" though somewhat complicated by the fact that every ontogeny has its beginnings in the preceding generation, that is in the oogenesis, preceding fertilization; if they are not determined in this way but are carried from generation to generation in the cytoplasm the inheritance is non-Mendelian. The fact that certain differential factors must be located outside the chromosomes will be considered further in the section on the "Mechanism of Development."

3. *Specificity of the Germ Cells*

One of the greatest advances in biological knowledge during the past century is found in the increasing recognition that vital units are specific and that the larger units of organization are themselves composed of many minor units having their own individuality and specificity. All cells, or "vesicles" as they were called by Wolff (1759), were at first supposed to be alike, and protoplasm was regarded as a uniform substance in all organisms; it was, in the language of Dujardin (1841), "Substance glutineuse, parfaitement homogène, élastique, contractile, diaphane. On n'y distingue absolument ancune trace d'organization, ni fibres, ni membranes, ni apparence de cellulositié." When this view was no longer tenable this simplicity and homogeneity were ascribed to germinal layers, embryonic cells and germ cells. Even within recent years cleavage cells

were said by Driesch (1893) to be all alike, "like balls in a pile," and differentiations were said to appear only late in development as the result of environmental conditions. So great was the influence of the doctrine of Epigenesis! Finally when this was disproved, the various constituents of cells such as cytoplasm, nuclei, chromatin, chromosomes, etc., were supposed to be alike in all cells.

Now, on the other hand, we consider that there are as many different kinds of protoplasm as there are different kinds of cells,—that one chromosome differs from another and that even chromosomes and genes are individually distinct and peculiar. Perhaps the most general and important discovery as to the mechanism of heredity is that germ cells are as specific as developed organisms, that "the egg of a frog differs from the egg of a hen as much as a frog differs from a hen"—indeed that just as every sexually reproduced animal or plant differs more or less from every other one so every germ cell differs more or less from every other germ cell.

Considering the vast numbers of germ cells which exist, their minute size and the difficulty of directly determining differences between them this statement may well challenge criticism and call for proof. And yet there is no gainsaying the fact that hereditary differences are due to germinal differences, and although no man can actually see the differences between the germ cells of different races or sometimes even of different species or genera, the results of development clearly demonstrate that such distinctions exist. In this case as in so many others physiological indicators are more delicate than morphological ones.

We have physiological proof that different chromosomes of the same cell differ in hereditary value and that different germ cells from the same individual frequently differ in hereditary constitution. In the separation of bivalent chromosomes at the reduction division each chromosome may go into either of the two daughter cells and the number of different combinations of chromosomes which may occur in the gametes is $(2)^n$, in which n is the number of bivalent chromosomes. Thus if there are 24 bivalent chromosomes, as in man, the possible permutations of these in the germ cells is 2^{24} or 16,777,216 and the possible number of different *combinations* of maternal and paternal chromosomes in the fertilized egg is this number squared or approximately three hundred thousand billions, though the actual number of *genotypes* is only 3^{24} or about thirty thousand billions, assuming that every chromosome differs hereditarily from every other one. Consequently it is not surprising that

successive children of the same parents are rarely if ever alike, since their chromosomal constitution is rarely if ever the same.

But the case for the specificity of germ cells is even stronger than this for there is reason to believe that particular chromosomes are not always composed of the same chromomeres (see p. 289) and the possible permutations of the many chromomeres of each chromosome furnishes an almost infinite number of combinations of units which are still visible with the microscope. Finally when we consider the possible combinations of still smaller units such as genes it is probable that every separate oosperm and every individual which develops from it is absolutely unique.

This conception of the specificity of gametes and zygotes sets the whole problem of the mechanism of heredity in a clear light. Unique individuals come from unique germ cells. Hereditary resemblances and differences in adult organisms are correlated with resemblances and differences in the germ cells from which they came.

C. Mechanism of Development

Development is progressive and coordinated differentiation by which the egg becomes the embryo and ultimately the adult organism. It is concerned with the manner in which the egg cell gives rise to different kinds of embryonic and tissue cells and with the way in which the latter form various cell products such as muscle and nerve fibrils, cartilage and bone. Development is also concerned with the coordination and integration of these differentiations so that an orderly arrangement of parts results. The mechanism of development, like the mechanism of heredity, is a cellular problem and it can be discovered only by the study of the cellular differentiations and integrations of development. But development, or the *transformation* of germinal units into developed characters, is a much more complex process than heredity, or the *transmission* of germinal units from one generation to the next, and at present relatively little is known as to the precise mechanism of such developmental transformations.

1. *Stability of Chromosomes in Development*

One of the most striking facts in ontogeny is that amid all the remarkable changes and differentiations which are taking place in cells their chromosomes remain almost entirely unchanged.

(*a*) *In Egg and Sperm.*—This is true of all kinds of cells but it is nowhere more striking than in the egg and sperm. These cells are notably unlike each other; the spermatozoon is one of the smallest of all cells, the egg is one of the largest, and correspondingly the entire nucleus of the spermatozoon is often smaller than a single chromosome of the egg; virtually all of the cytoplasm of the sperm is converted into a complicated locomotor apparatus whereas the cytoplasm of the egg is abundant, relatively undifferentiated and contains much yolk. It is certainly no exaggeration to say that the sperm and egg differ as much as muscle and nerve cells or as connective tissue and gland cells. And yet in spite of this great difference between the two germ cells their chromosomes are so much alike after they have united that it is usually impossible to distinguish them. The extreme differentiation of the spermatozoon has not in any way changed its chromosomes for they issue from it exactly as they went into it, the same in number, shape and even in genetic constitution in spite of the fact that they have been compressed into one of the smallest of all nuclei in one of the most highly differentiated of all cells.

(*b*) *In Tissue Cells.*—The chromosomes of tissue cells are, in many cases, precisely like those of the oosperm. The differentiations which the cell body has undergone do not usually modify the chromosomes. This is especially true of embryonic cells where mitoses are more abundant and more easily studied than in fully developed tissue cells. In Orthoptera, according to McClung, not only the number of all the chromosomes but even the shape of each particular chromosome is constant for all cells of a given individual, though they may differ slightly in different individuals of the same species. Hoy found that the numbers and shapes of chromosomes in embryonic muscle, nerve, gut and connective tissue cells of certain insects were the same as in the fertilized egg.

On the other hand, Miss Holt found that the number of chromosomes in the alimentary tract of larval mosquitoes might vary from 6 to 72 but always in multiples of 3, the haploid number in this species. She concludes therefore that these variations in number are due either to a double splitting of each chromosome in certain mitoses or more probably to the failure of the cell to divide after the chromosomes have divided. The latter is a phenomenon which not infrequently occurs in cells subjected to experimental conditions. Hance found in the pig that the somatic chromosomes of many cells are more numerous than the diploid number but he has proved that

this is due to fragmentation of chromosomes, the fragments when added together merely equalling in length the original chromosome. Such fragmentation of chromosomes in somatic cells was first observed by Boveri in the case of *Ascaris megalocephola* var. *univalens* (Fig. 23). Here the diploid number of

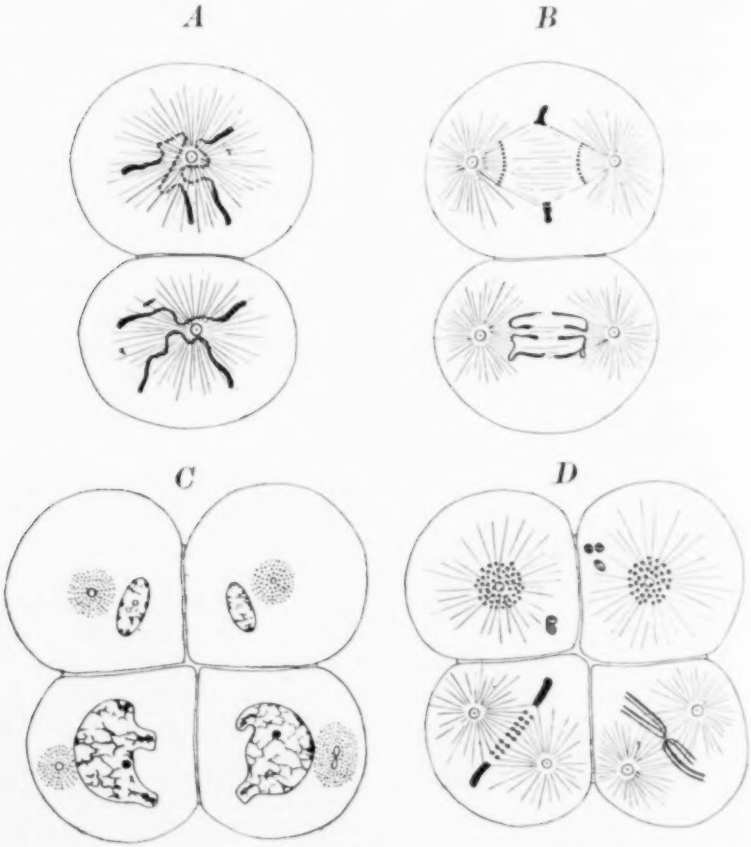


FIG. 23. DIFFERENTIATION OF CHROMOSOMES OF SOMATIC CELLS IN *Ascaris megalocephala*, var. *univalens*. *A* and *B*, Second cleavage mitosis showing the chromosomes entire in the lower cells (germ track); in the upper (somatic) cells their ends are thrown off and the remainder breaks up into many small chromosomes. *C*, 4-cells, the upper two (somatic cells) containing small nuclei derived from the small chromosomes. *D*, Third nuclear division, showing the somatic differentiation of the chromosomes in all the cells except the lower right one, which alone is in the germ track and will give rise to sex cells. (After Boveri.)

chromosomes is 2 but in somatic cells more than 30 small chromosomes (or chromomeres) are present, the total volume of which is even less than that of the 2 original chromosomes since the ends of these chromosomes are cast off and dissolved in the cytoplasm (Fig. 23, *C*, *D*). Boveri has proved by exper-

iments that whether chromosomes fragment or not depends upon whether they lie in a particular field of differentiated cytoplasm and consequently that this is due to the influence of cytoplasm on the chromosomes, and not the reverse.

In conclusion, the chromosomes of tissue cells, as well as those of germ cells, are usually unmodified by the differentiations of the cell body. In other cases these chromosomes may be fragmented or possibly otherwise modified by the differentiations of the cell body. But there is no evidence that the differentiations of these chromosomes are primary and that they cause the differentiations of the cell body as Weismann assumed.

2. *Differentiation of Cytoplasm in Development*

The cytoplasm is the chief, perhaps in some cases the only, seat of differentiation in the developing organism. Practically all the differentiations of cells, tissues, organs and persons are differentiations which arise (1) by transformations of cytoplasm (autoplasmatic differentiation) or (2) as secretion products of cytoplasm (apoplasmatic differentiation).

a. The Structure of Cytoplasm in General.—All students of the cell now agree that protoplasm is composed of a more fluid and a more solid portion, though there is much difference of opinion as to the form of each of these and their relations to each other, as is shown by the various theories of the "structure of protoplasm." The more fluid part is often called enchylemma or cytolymph, the more solid part spongioplasm or kinoplasm; the latter forms some sort of a framework, in the form of fibers, or a net-like or a foam-like structure while the former fills the interstices which are left. Recent experiments on cells and protoplasm indicate that these two portions may vary in consistency at different phases of cell activity, such variations being perhaps in the nature of reversible sols and gels.

But whatever the relation may be between these portions of protoplasm, experiments in which cells are subjected to strong centrifugal force indicate that the framework of the cell is a viscid, elastic, contractile gel which is more rigid at certain phases of the cell cycle than at others, depending probably upon its water content, and that within this gel are included water, oil, yolk, pigments, granules and other products of differentiation.

In addition to this general framework of spongioplasm there is present in many cells, especially at the time of division, a special structure, the mitotic figure or amphiaster, consisting probably of an elastic, contractile gel which takes the form of

a spindle with star-shaped radiations at its two poles. This amphiaster is undoubtedly an apparatus for the division of the nucleus and cell body, but we do not know exactly how it functions, though it probably brings about intra-cellular movements, together with segregations and localizations of different cell substances.

b. Formation of Different Substances in Cytoplasm.—In the course of development different substances are formed in different parts of cells or in different cells. The exact manner in which these substances are formed is a matter of great interest in connection with the mechanism of differentiation and development. Unfortunately our knowledge upon this point is very incomplete. The general evidence seems to favor the view that the nucleus cooperates in the formation of the various differentiation products which appear in the cytoplasm; in general these substances are not formed when the nucleus is absent though once differentiation of cytoplasmic substances has begun it may go on for a time in the absence of the nucleus. The manner in which the nucleus cooperates in differentiation has been variously interpreted by different authors. Weismann assumed that in mitosis there was a differential distribution of the nuclear inheritance factors to the different cells and that in this way the differentiations of development were to be explained. The evidence is all against this view as Boveri and others have shown. In the first place the splitting of chromosomes is never differential and the mitotic apparatus is unsuited to a differential separation of daughter chromosomes. Only in the separation of whole chromosomes which had previously united in pairs, as in the maturation divisions, is such a differential separation possible. In the second place the experiments of Driesch and O. Hertwig on compressed frog's eggs and of Conklin on Ascidian eggs proves that nuclear divisions are not differential, but that cytoplasmic divisions often are. Finally the differentiation of the somatic chromosomes of *Ascaris* (Fig. 23) is due to the fact that they lie in a field of peculiar cytoplasm as Boveri has shown; consequently such chromosomal differentiations are the result of cytoplasmic differentiations and not their cause.

(1) *"Intracellular Pangenesis."*—Under this name deVries (1889) proposed a hypothesis of the way in which the nucleus controls differentiation which nearly meets present requirements and fits present knowledge. He suggested that particles which he called "pangenes" and which he regarded as enzymatic in character escape from the nucleus into the cell body

where they form the entire living protoplasm. Most of the nuclear pangenes are inactive but they may be activated by age or outer circumstances. Every nucleus contains a full set of pangenes and there is no differential division of them in mitosis. The pangenes which escape from the nucleus must be transported to different parts of the cell and segregated at the right places and this transportation and localization is brought about by the streaming of the protoplasm.

In its main features this hypothesis is acceptable to-day. However deVries's conception that "every inherited character has its particular kind of pangene," has suffered the same fate as the more recent view that "determinants," "determiners" or "inheritance factors" are the germinal representatives of developed characters. These are not the germs of developed characters any more than oxygen and hydrogen are the germs of water; on the other hand they are only specific causes which, acting in conjunction with other causes, produce specific results.

Furthermore it is not now believed that genes or pangenes maintain their identity in the cytoplasm and that the entire cytoplasm is composed of them. It is not even known that genes escape *as such* from the nucleus into the cytoplasm but it is known that there is an escape of nuclear substances into the cytoplasm and that as a result of syntheses of these with portions of the cytoplasm new substances are formed which were not present before.

(2) *Escape of Nuclear Substances into Cell-body*

(*a*) *At Mitosis.*—Every cytologist is familiar with the fact that various nuclear substances escape into the cell-body. One of the most striking instances of this occurs during mitosis when the nuclear membrane is dissolved and its contents are set free into the cell. In some cases the entire volume of nuclear materials which are thus set free has been estimated to be five-hundred times as great as the volume of the chromosomes which give rise to the daughter nuclei. Among the nuclear materials which thus escape are nuclear sap, linin, nucleoli and oxychromatin. The last is probably derived from the chromosomes and it takes part in the formation of the "sphere substance" which surrounds the centrosomes. This sphere substance is differentially distributed to different cleavage cells as for example in the case of Crepidula (Conklin, 1902), and it probably cooperates in the differentiations of these cells.

The immediate results of the escape of nuclear materials into the cytoplasm are both varied and striking. At once there

is greatly increased oxidation with the formation of carbonic acid, and the cell which was before relatively stable and resistant becomes unstable and peculiarly liable to injury.

Another result is the rapid formation of astral rays and spheres around the centrosomes; indeed very much of the cell cytoplasm may be transformed into these structures following the escape of nuclear material during mitosis.

Another notable result of this escape of nuclear material is found in the movements which are set up in the cell and which lead to the separation of daughter chromosomes, the orientation of spindles, the localization of different cytoplasmic materials and finally the division of the cell-body. All of these movements, which may be collectively known as karyokinesis and cytokinesis, start with the escape of nuclear material into the cytoplasm.

(*b*) *Chromidia.*—R. Hertwig and his associates maintain that chromatin may escape through the nuclear membrane into the cell-body during resting stages. Such escaped chromatin forms chromatic granules which they call "chromidia" and which they believe take part in the differentiation of various intracellular structures, such as skeletal, muscle- and nerve-fibrils, secretion granules, pigment and the Nissl-bodies of nerve cells. However it is not certain that chromidia are derived from escaped chromatin.

(*c*) *Mitochondria.*—Other observers, particularly Meves and Duesberg, have described granules, rods or threads in the cytoplasm which are known by the general name of "mitochondria" and which appear to take part in the differentiations of specific structures. Meves and Duesberg regard these as purely cytoplasmic bodies which have the power of growth and division. Hertwig and his school hold that they are derived from chromidia, and therefore in the beginning from chromatin; more probably they are new formations caused by the action of chromatin on cytoplasm.

(3) *Cytoplasmic Differentiation*

One of the simplest cases of differentiation is found in the formation of secretion products within cells, such as yolk, oil, and zymogen granules. These usually appear as minute granules or droplets which then grow in size until they more or less completely fill the cell. Whether they are products of destructive or of constructive metabolism is not altogether clear; probably in some cases they are the former, in others the latter.

Yolk and zymogen begin to form in the vicinity of the

nucleus and in many cases out of a granular mass which is either chromidia, mitochondria or the granular substance surrounding the centrosomes and known as "sphere-substance." In some cases, perhaps in all, the granular body known as a "yolk nucleus" is sphere-substance derived from the interaction of nucleus and cytoplasm; according to the Hertwig school zymogen granules are always derived from chromidia and pigment usually comes from the same source.

Intra-cellular fibrils, such as skeletal, muscle- and nerve-fibrillæ, are also derived from chromidia, according to Goldschmidt, and hence are formed in large part by substances derived from the nucleus. On the other hand, Meves and Duesberg maintain that such intra-cellular differentiations are derived from mitochondria which are purely cytoplasmic in origin. The more probable view is that mitochondria are formed by the interaction of nucleus and cytoplasm, and that all other cellular differentiations are formed in the same way; and if all these cytoplasmic differentiations are produced by the action of chromatin on cytoplasm, the chromatin is only one factor in their origin.

As a result of all these observations it is impossible to avoid the conclusion that the nucleus is intimately concerned in differentiation, and the mechanism of the "nuclear control" of the cell is at least suggested by the escape of chromatin into the cytoplasm and the formation there of various differentiation products such as mitochondria, sphere substance, fibers, granules, etc.

(*c*) *Segregation and Isolation of Different Substances.*—Embryonic differentiation involves not only the formation of different substances in cells, but also their segregation in particular parts of cells and ultimately, in most cases, their isolation in different cells. Such segregation and isolation are seen especially well in the cleavage of the egg. By the flowing movements of cytoplasm during mitosis the various substances in the cells are oriented and sorted and by the formation of division walls between daughter cells these substances become permanently isolated. Such segregation and isolation of different cytoplasmic substances is certainly one of the most important functions of cleavage.

(1) *Differential and Non-Differential Cell Divisions.*—Cell dixisions are plainly of two kinds, differential in which the daughter cells are unlike in size or contents, and non-differential in which they are alike. Both of these occur in the cleavage of the egg but only the former contributes directly to

embryonic differentiation. Given a cell which is not homogenous, or in which every radius is not like every other one, it follows that cell divisions will be differential or non-differential depending upon the position and direction of the cleavage planes. Under normal conditions these planes are constant in position and they follow one another in a definite sequence just as do all processes of development. Consequently the pattern and character of the cleavage and its relation to differentiation is nearly constant for each species. Any explanation of the causes of differential cell-division must account for the localizations of different materials in cells and for the orientation of the planes of cell division.

(2) *The Orientations of Development.*—All of the orientations of development find their earliest visible expression in the polar differentiation of the egg. This polarity determines not only the polarity of cleavage cells, embryos and adults but it is also causally related to the direction of movement of the germ nuclei and of cytoplasmic substances, and consequently to the type of symmetry and the pattern of localization, as well as to other orientations of development. The problem of the causes of these orientations is perhaps the greatest problem of embryogeny.

Experiments with eggs subjected to centrifugal force indicate that pigment, oil, yolk and other inclusions are passively localized in certain parts of the cell and that the substance in which polarity persists is the elastic, contractile spongioplasm, which differs in structure or consistency at different poles and at different stages of the cycle of division. Protoplasmic flowing may be best explained as the result of the contractility of the spongioplasm, and the definite localization of inclusions, mitotic spindles and division planes, as caused by the polar differentiations of the spongioplasm. This polar differentiation of the spongioplasm persists and to it are to be referred in the last analysis many if not all of the orientations of development.

(3) *The Chromosome Theory of Heredity Applied to Embryonic Differentiation.*—According to the chromosome theory of heredity the inheritance factors are located in the chromosomes, and the cytological evidence shows that chromosomes always divide equally and presumably every cell of an individual contains the same kinds of chromosomes and the same kinds of inheritance factors. How then is it possible to explain embryonic differentiation? How can identical factors give rise to different products in different cells?

This is evidently due to the fact that while the division of chromosomes is non-differential, that of the cell body is often differential and the same chromosomes and genes acting upon different kinds of cytoplasm will produce different results (Fig. 10, p. 54). But differential cell-division is the result of definite movements in the cytoplasm, of definite orientations of spindles and cleavage planes, and ultimately of a definite polarity and symmetry of the cytoplasm. There is abundant evidence that these cytoplasmic orientations are not the immediate results of chromosomal activity and even if some of them may be the remote results of such activity it is logically impossible to place all the differential factors of development in non-differentiating genes.

On the other hand if embryonic differentiations are produced by the interaction of chromatin and cytoplasm, and if the chromatin does not undergo differentiation, it follows that some of the differential factors of heredity and development must be located in the cytoplasm. Such factors would probably not be genes and would not be transmitted in Mendelian fashion, but they would need to be present in the cytoplasm from the very beginnings of ontogeny. They need not be numerous—in fact they are probably few in number—but they are absolutely indispensable to development. If a few orientating differentiations such as polarity and symmetry are present in the cytoplasm at the beginnings of ontogeny all other differentiations of development can be explained as due to the interaction of non-differentiating genes on different parts of this cytoplasm, but there is no mechanism by which embryonic differentiations could come from the action of non-differentiating genes on a homogeneous cytoplasm. The genes or Mendelian factors are undoubtedly located in the chromosomes and they are sometimes regarded as the only differential factors of development, but if this were true these genes would of necessity have to undergo differential division and distribution to the cleavage cells; since this is not true it must be that some of the differential factors of development lie outside of the nucleus and if they are inherited, as most of these early orientations are, they must lie in the cytoplasm.

N. R. Graves, Photo.

THE ROCHESTER CANYON. View looking south, upstream, toward Driving Park Avenue bridge.

A NATURE DRAMA

By Professor H. L. FAIRCHILD

UNIVERSITY OF ROCHESTER

The Place ..... New York State.
The Time ..... The Pleistocene: some little while ago.
Principal Characters:
A vast Ice Sheet ..... Quebec Glacier.
A Lake ..... Dawson.
A second Lake ..... Iroquois.
A third Lake ..... Ontario.
Two sister Rivers ..... Niagara and Genesee.
Attendant Rivers ..... Dawson Outlet; Iromohawk; Covey; St. Lawrence.
Oceanic Waters ..... Hudson-Champlain; Gilbert Gulf.
An invisible, subterranean Force ..... Diastrophism.[1]
Attendant Atmospheric Agents ..... Epigene.
Solar Energy ..... Sun Heat.

PROLOGUE

Joseph Rodman Drake wrote "The Culprit Fay" with the purpose of producing a dramatic story without human characters. But his characters imitate humans, and the motif and movement of the story are simulation of human action. The present dramatic narration has no human relation whatever. Furthermore, it is not in the least imaginary, but is a recital of actual events, the conflict of the forces of Nature and the interaction of material agents which shape the surface of the globe. The interplay of Nature's forces and processes are as truly dramatic as anything in human action and emotion; and as the milleniums pass the human play seems petty and trivial by contrast with the changes in the earth and the cosmos. And, after all is said, Man is only a phenomenon of nature.

The reader asks for the *purpose* in the drama—the motive which impels the characters. If the reader can tell the purpose of the universe and its activities, of the motive in tide and storm and earthquake, and in the making and shaping of the continents, then he will know the purpose which animated the characters of this drama.

The story here given is merely a translation of the many conspicuous records left by the *dramatis personæ*. The in-

[1] Description of this character will be found in a ponderous volume called Webster's Dictionary.

ROCK STRATA, ROCHESTER CANYON. View looking north from Driving Park Avenue bridge.

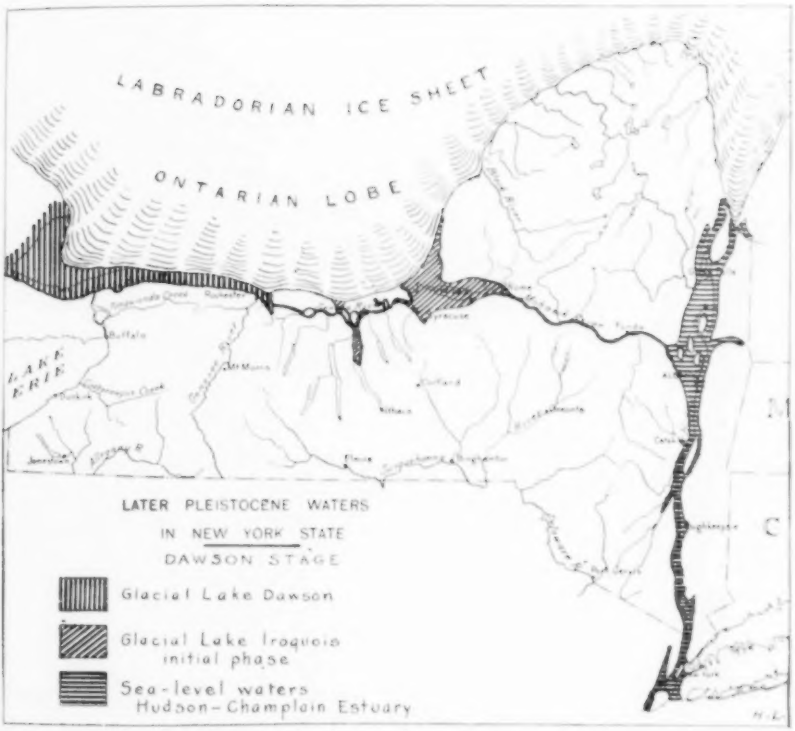


FIG. 1. DAWSON STAGE OF THE GLACIAL WATERS.

scriptions left by the glacier, the rivers, the lakes and seas, are open to observation and are not difficult of interpretation.

The portion of the great drama which is here given covers only the latest episodes in a long series of dramatic and romantic events which have been enacted on the stage of New York State. Some of the actors are yet alive and playing their parts. The earlier scenes have been partially described in the dramatic (geologic) literature.

The story may be epitomized as follows: The forces and processes of the atmosphere, here called Epigene, had created a vast ice sheet, central in Quebec, which had overridden the entire area and had long occupied the stage to the exclusion of the other characters. This dominant performer was now grudgingly yielding room to the other actors, under the compulsion of Sun Heat. Other characters appear on the stage, "strut their little day" (geologically speaking), while some of them have passed away into oblivion. The unseen "villain" of the play, Diastrophism, hereafter called Diastro, is constantly disturbing the equilibrium of the stage and interfering

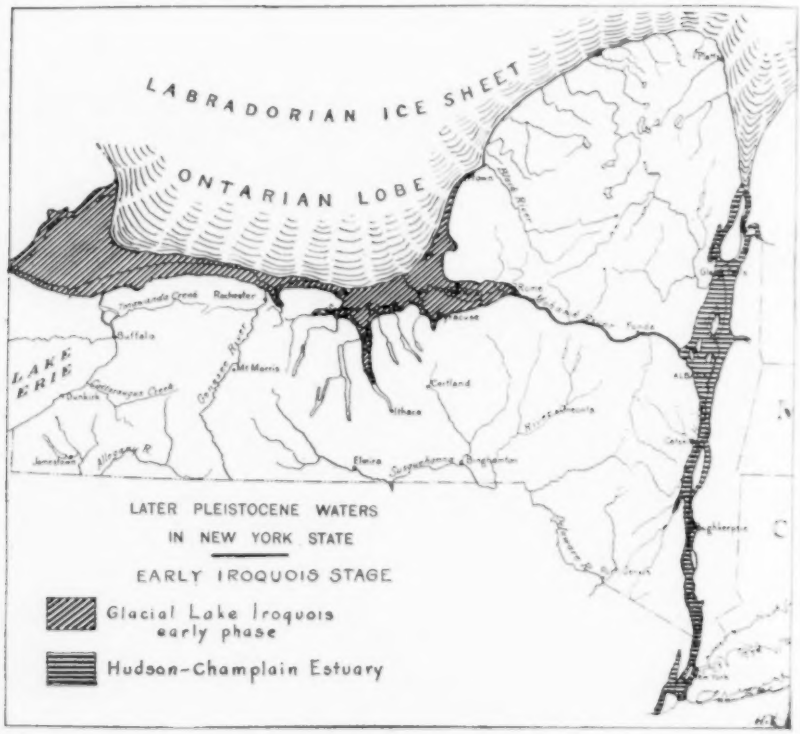


FIG. 2. EARLY IROQUOIS STAGE.

with the visible actors, by raising and tilting the entire stage. This change of level, accompanied by the withdrawal of Quebec Glacier, causes rivalry and changes in the standing waters, the Lakes, and in the appearance and disappearance of the running waters, the attendant Rivers. Finally Quebec Glacier makes its exit and the aqueous and atmospheric performers take the present-time position and relation.

ACT ONE

Scene 1

The stage setting is shown in the diagram, Fig. 1.

Color scheme: Glacier, white; Rivers, green; Lakes, blue.

Quebec Glacier holds the center of the stage.

Enter, Dawson Lake, with Iroquois, and the attendant Montezuma Lake.

Movement

The Glacier, the cold, impassive member of the dramatic group, has been losing power and control, but yet holds the

contiguous waters at high levels. Dawson Lake, the successor of a long sequence of glacier lakes, is confined to the western part of the stage, in the Ontario basin, having altitude 230 feet above the sea. Into this lake pour two rivers, Niagara and Genesee. The former is very young and is now making its debut. The latter is of great age with long life on the stage. Niagara carries, as today, the outflow of the Great Lakes. The Niagara Falls and the upper Rochester cataract are in early life.

The Dawson Outlet River, flowing through Fairport, Palmyra, Newark and Lyons, carries the copious waters eastward, grading a path for human use. This river loses itself in Montezuma Lake, lying in the low ground of Seneca and Cayuga valleys, which lake is itself drained into the young Iroquois Lake, lying in the Syracuse-Rome district. Iroquois Lake has altitude 110 feet above the sea, and outflows at Rome by the Iromohawk (abbreviation for Iroquois-Mohawk) River to final repose in oceanic waters in the Schenectady-Albany district.

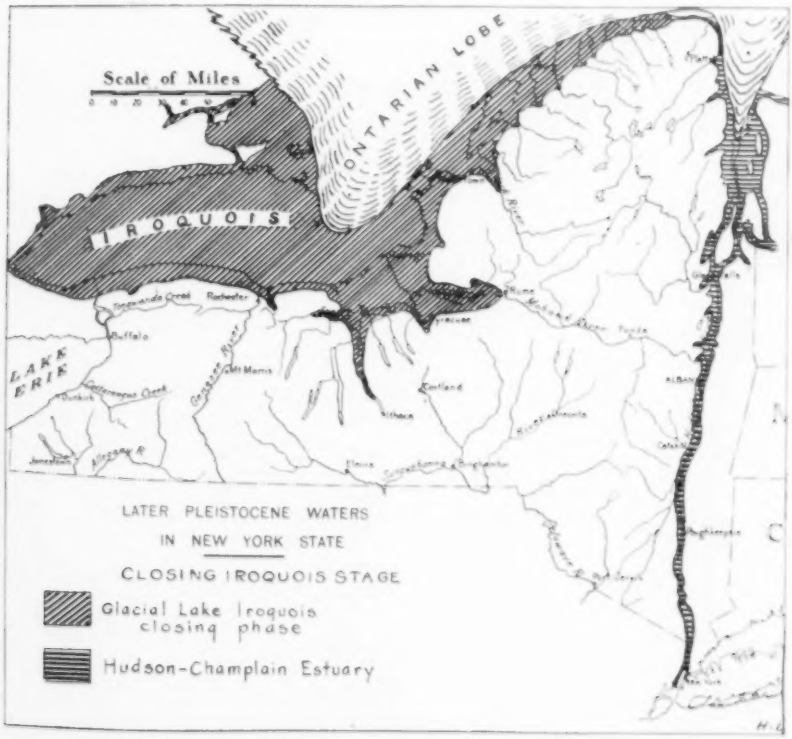


FIG. 3. CLOSING IROQUOIS STAGE.

FIG. 4. GILBERT GULF, SEA-LEVEL WATERS.

These great rivers are now grading the lowest path from the Atlantic seaboard to the Great Lakes and the Mississippi Valley.

All the great area which stages this scene was much lower in altitude than it is to-day. The Iromohawk delta, the sand-plain between Schenectady and Albany, was then building at sealevel, or 350 feet lower than to-day. Rome was 350 feet and Rochester 250 feet lower than now. Diastro, the strong villain of the play, begins his slow but irresistible business of lifting the stage, producing the changes in level.

The Oceanic Waters of the Hudson Estuary occupy the slowly rising Hudson Valley.

Closing the scene, Lakes Dawson and Montezuma and their attendant Rivers, exeunt.

Scene 2

This scene is depicted in the diagram, Fig. 2.

Owing to the pressure of Sun Heat the Glacier yields a few miles and the young Iroquois Lake supplants two lakes and attendant rivers of the preceding scene. Iroquois now extends

the whole length of the Ontario Basin, from Rome, N. Y. to Hamilton, Canada.

With Iroquois succeeding Dawson the base-level of the Rivers Niagara and Genesee, and all the minor streams, falls from 230 feet above ocean to 110 feet, and the rivers extend their courses northward to the lower waters (see Figs. 7, 8).

Up to this time Diastro has not seriously affected the progress of the drama, the movement of the play being chiefly due to the weakening and withdrawal of Glacier.

Iroquois Lake clings to Glacier, and expands as the latter wanes.

ACT TWO

Scene 1

The stage setting is shown in Fig. 3.

Exit Iromohawk River.

Sun Heat and Epigene have pressed Quebec Glacier nearly off the stage. At only one point does the latter now interfere

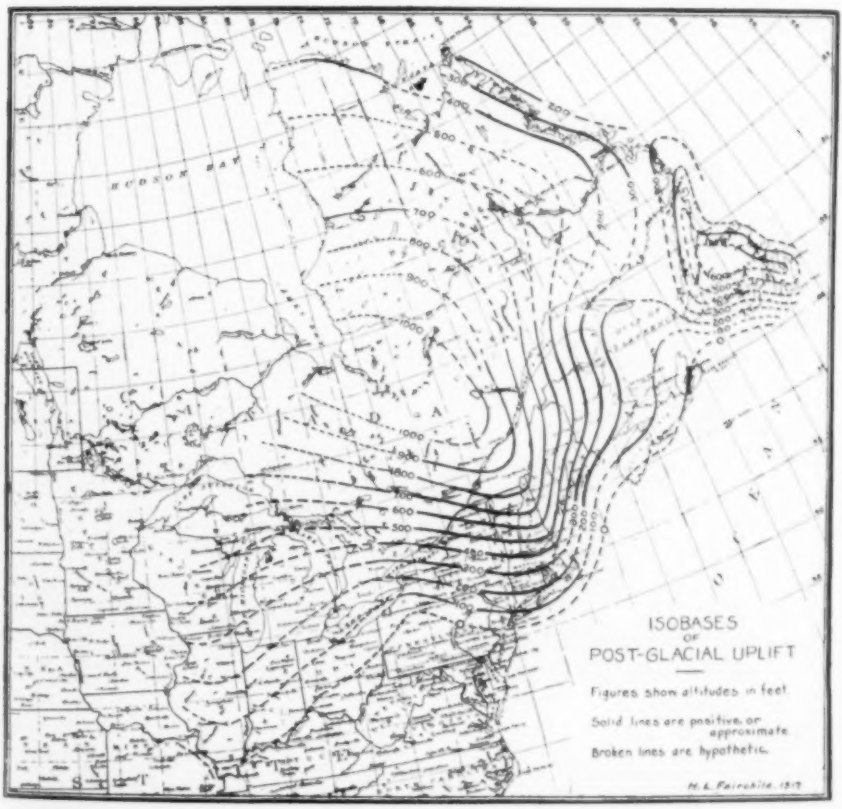


FIG. 5. UPLIFT OF NORTHEASTERN AMERICA.

Upper Falls, Genesee River, Rochester, N. Y.

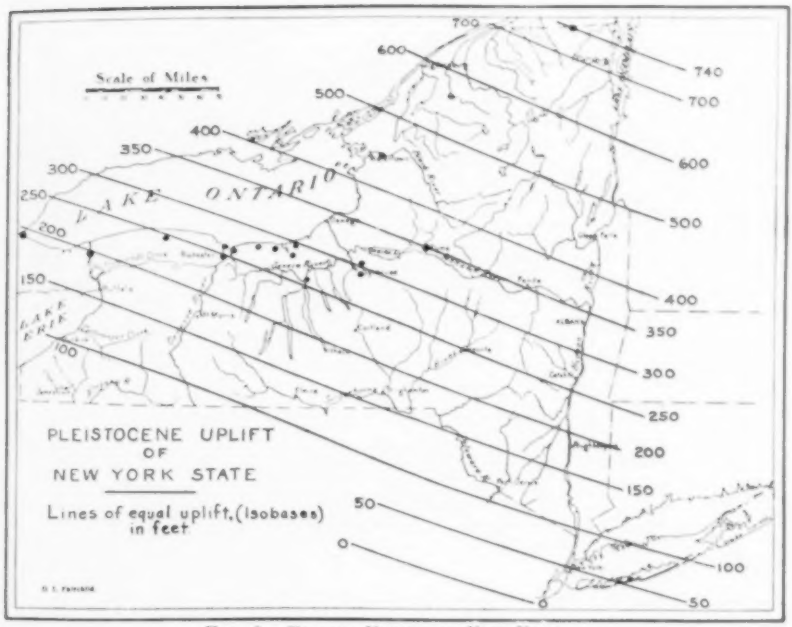


FIG. 6. TILTING-UPLIFT OF NEW YORK.

with the other performers. On the Canadian boundary of New York the Glacier is holding the outflow of Iroquois Lake to a new channel, the Covey pass. The Covey River, on account of lower altitude, is the successor to Iromohawk, carrying Iroquois water to sealevel in the Hudson-Champlain estuary.

Iroquois Lake is at its maximum.

Scene 2

The stage setting is the same as in scene 1.

Quebec Glacier is waning. Iroquois Lake is about to retire. Diastro is the leading performer (Figs. 5, 6).

By the tilting uplift, and the land rise at Rome of 180 feet, Diastro has raised the level of Iroquois Lake from 110 to 290 feet.

As the land at Rochester has risen only 105 feet the Iroquois water produces a flooding there of 75 feet. The rivers Niagara and Genesee, and their companions, are checked in flow by the rising lake, and their mouths are forced back, up the land slopes (in Figs. 7, 8 from position 2 to position 3).

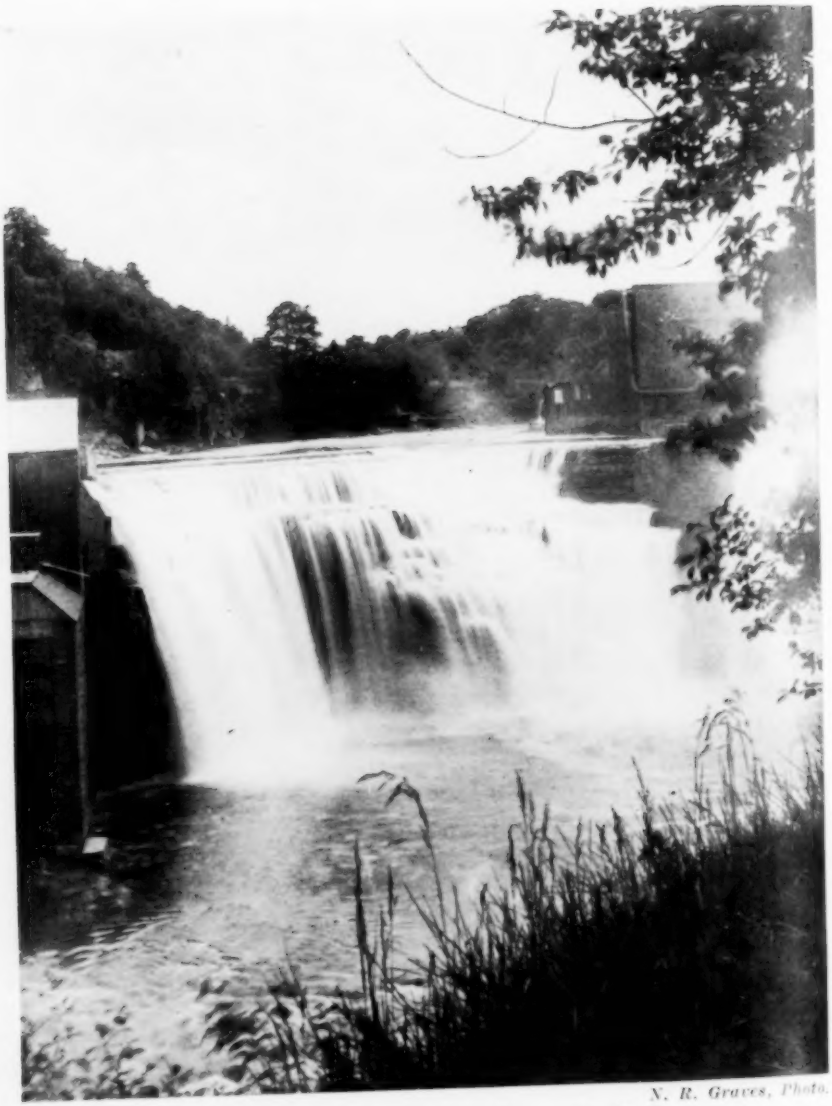
Exit Iroquois. For New York State the Glacial Period has ended.

ACT THREE

Scene 1

The stage setting is shown in Fig. 4.

Upper Falls, Genesee River, Rochester, N. Y.



N. R. Graves, Photo.

LOWER FALL, GENESEE RIVER, ROCHESTER, N. Y.

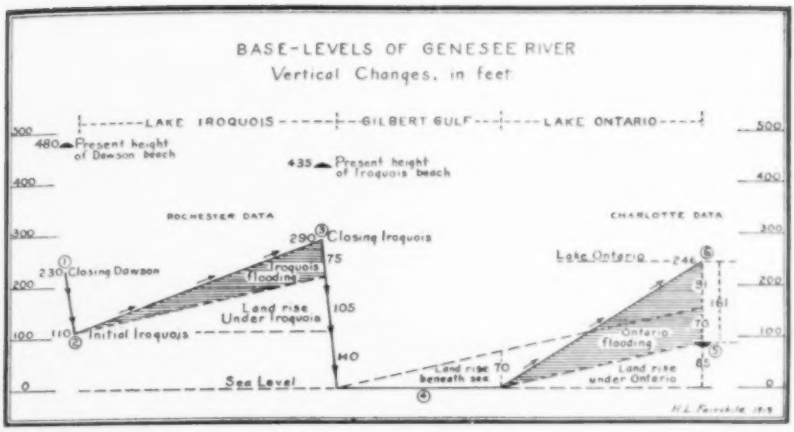


FIG. 7. VERTICAL CHANGES IN THE GENESEE RIVER BASE-LEVELS.

Enter, ocean-level water in the Ontario basin, Gilbert Gulf.

The frozen, damming member of the company has removed from the stage (the area of New York) and stands back in the wings.

Relieved of all interference the water in the Ontario basin falls to sealevel, and Gilbert Gulf succeeds Iroquois Lake. The base-level of all the rivers falls from 290 feet to zero, and the rivers extend themselves far northward to the sealevel water. (In Figs. 7, 8 the mouth of the Genesee shifts from position 3 to position 4.)

Scene 2

Stage setting the same as in scene 1.

The performers are few—the sealevel waters, Hudson-

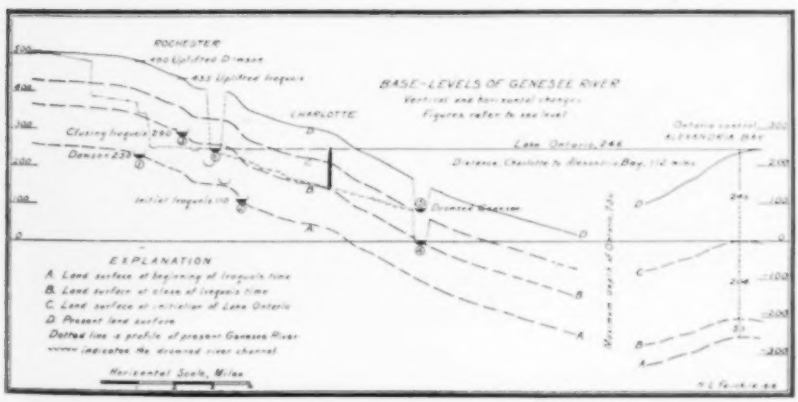

ROCHESTER
CHARLOTTE
BASE-LEVELS OF GENESEE RIVER
Lake Ontario, 246
EXPLANATION
A. Land surface at beginning of Iroquois time
B. Land surface at close of Iroquois time
C. Land surface at initiation of Lake Ontario
D. Present land surface
Dotted line is profile of present Genesee River
Horizontal Scale, Miles

FIG. 8. VERTICAL AND HORIZONTAL CHANGES IN THE GENESSEE RIVER BASE-LEVELS.

G. H. Chadwick, Photo.

THE ROCHESTER CANYON. View looking south, upstream, toward the city. The river at level of Lake Ontario, 246 feet.

Champlain estuary and Gilbert Gulf; the rivers Niagara and Genesee and their attendants; Sun Heat and Epigene with attendants.

The movement is chiefly the action of Diastro, in lifting the northern part of the stage more rapidly. But until the district of the Thousand Islands is raised out of the sealevel waters there is no change of scene.

Scene 3

The setting of the stage is any map of the present geography of New York State.

Exit, Gilbert Gulf.

Enter, St. Lawrence River and Lakes Champlain and Ontario.

Diastro erects a barrier in the St. Lawrence Valley, at the Thousand Islands, which imprisons the waters of Gilbert Gulf and transforms them into Ontario Lake. The uplifting of the barrier continues until Ontario water is 246 feet above the sea.

The embouchures of the rivers are pushed backward, up the land slopes, as had been the case during the rise of Iroquois. (In Figs. 7, 8 the mouth of the Genesee is shifted from position 4 to position 6.)

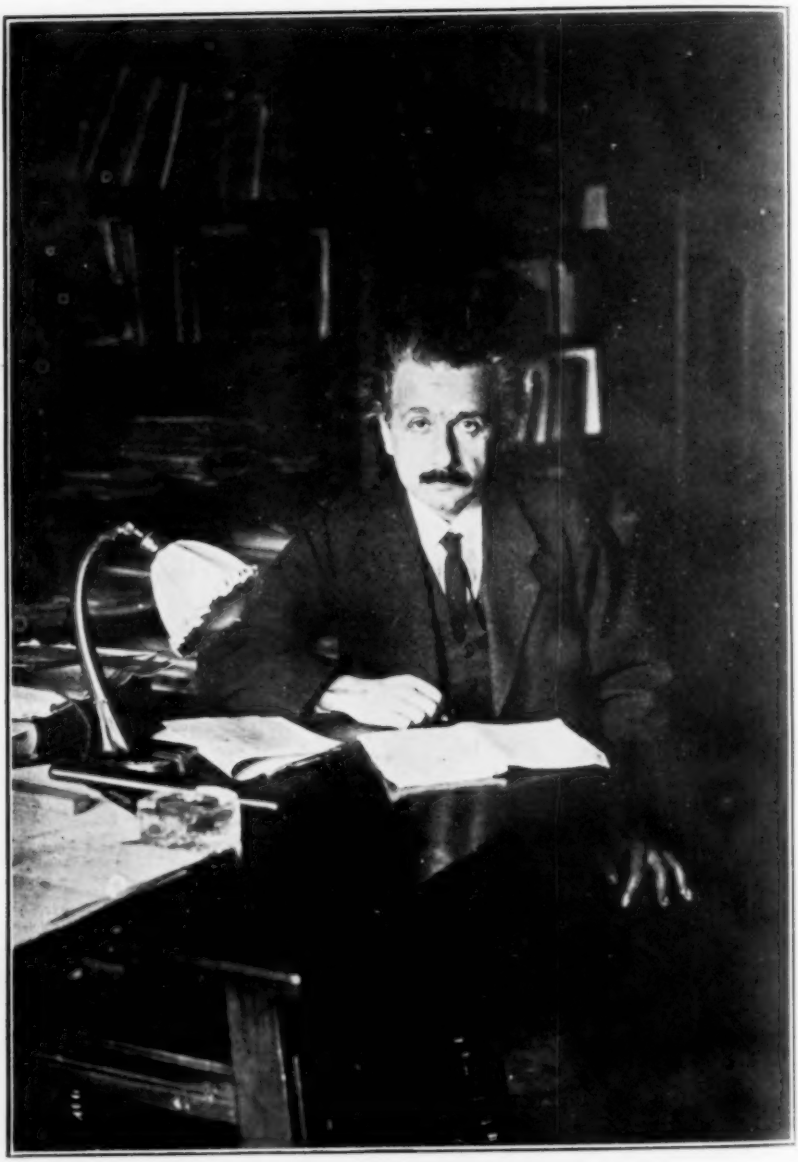
As Charlotte, the location of the present embouchure of Genesee River, rises only 85 feet while Ontario level rises 246 feet, the flooding by Ontario water has drowned the river channel that was cut in time of Gilbert Gulf (Fig. 4) under 161 feet of water. (The drowned position is indicated by position 5 of the diagrams, Figs. 7, 8).

The quantitative vertical elements showing the total work of Diastro are given in Figs. 5, 6, 7 and 8.

EPILOGUE

Here ends the translation. But the actual drama does not end with this brief relation. The drama is yet in progress, superior to any human cooperation or opposition; and it will continue for millions of years after humanity has run its course and disappears from the earth. The movement of the play takes no note of time. In the far future the stage of New York area may carry changes as dramatic, or perhaps greater, than those recorded in the immediate past.[1]

[1] The detailed story of the events here epitomized is published in *Proceedings of the Rochester Academy of Science*, Volume 6, 1919, pages 1–55, and the general history is given in Bulletin No. 209–210 of the New York State Museum.



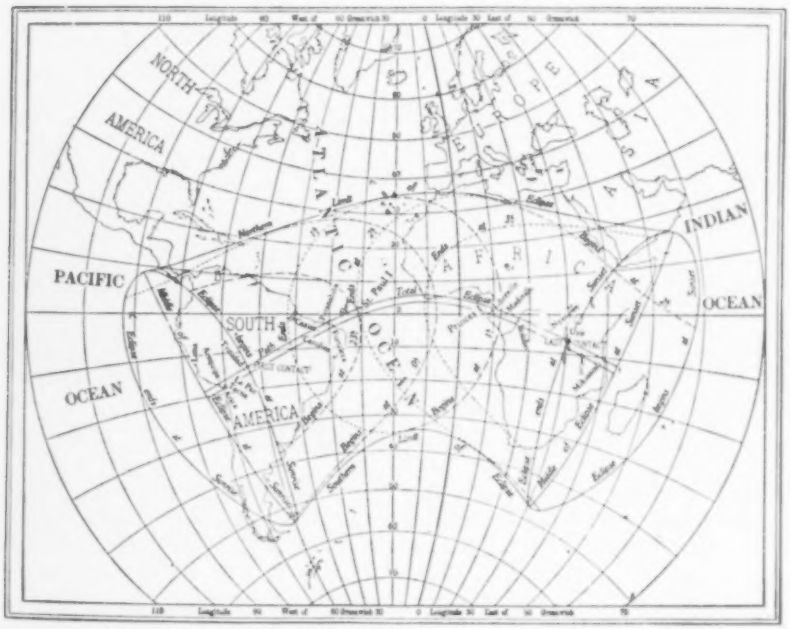
Professor Albert Einstein,
University of Berlin.

THE PROGRESS OF SCIENCE

THE SOLAR ECLIPSE OF MAY 29, 1919, AND THE EINSTEIN EFFECT

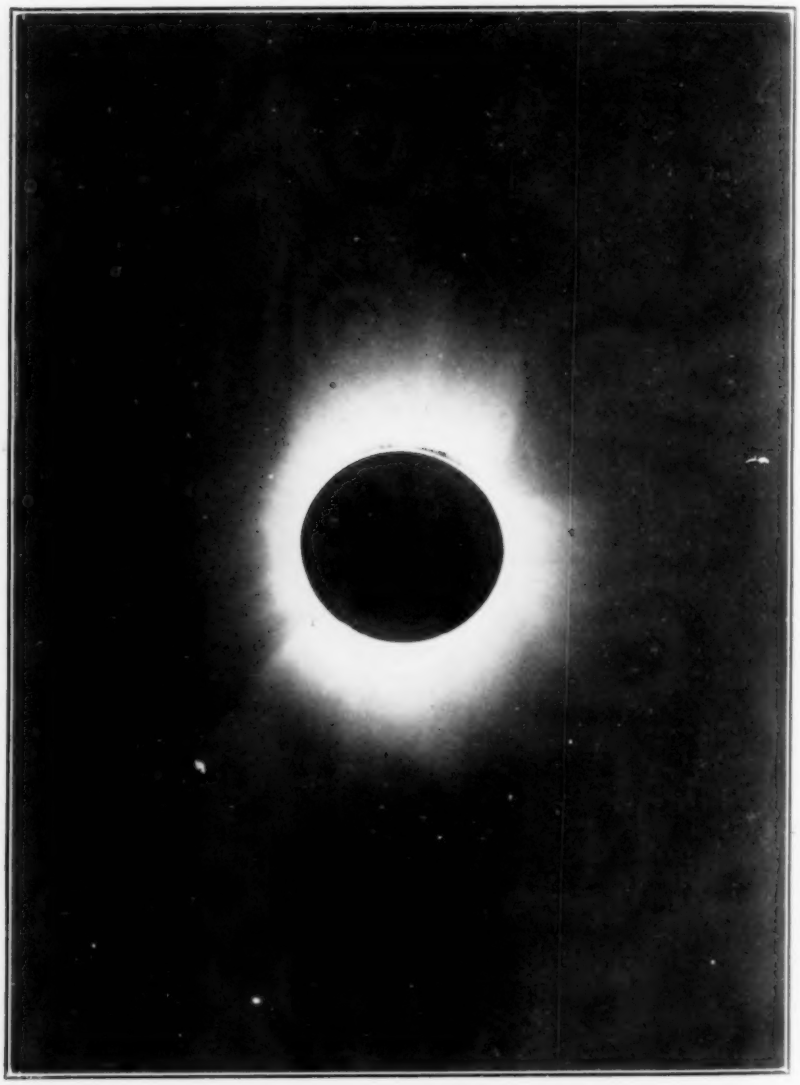
THE supposed verification, by British astronomers during the solar eclipse of May 29, 1919, of the Einstein prediction that rays of light, while passing near the sun, would be bent out of their straight course by the sun's gravitational pull, like any flying projectile, has made, says Dr. L. O. Bauer,[1] an eclipse of the sun of more than passing interest, not alone to the astronomer, but also to the geophysicist, the mathematician, the physicist, and, in fact, to the philosopher, in general.

If a star-ray, on its way to the earth, just grazed the sun's limb, then the star, as seen by a terrestrial observer, or caught on a photographic plate, would either not be displaced at all, or it would be displaced by a certain minute amount, a. If the principles of the Newtonian mechanics hold for particles moving with the velocity of light (186,000 miles a second), then on the basis of Maxwell's electromagnetic theory of light, the apparent displacement, a, away from the sun would amount to 0″.87; this was the shift predicted by Einstein in his first theory of relativity. According to his later or generalized theory of relativity, which brings under the same purview electromagnetic and gravitational phenomena, the star would be displaced apparently by the amount $2a$ or 1″.74. If the ray of light passed through the sun's gravitational field at the distance from the



THE PATH OF THE TOTAL SOLAR ECLIPSE OF MAY 28–29, 1919.

[1] See résumé of public lectures given by him before various societies and universities, published in the issue of *Science* for March 26.



THE SOLAR CORONA DURING THE ECLIPSE OF MAY 29, 1919, as photographed by Dr. C. G. Abbot, of the Smithsonian Institution, at an elevation of 14,000 feet, near La Paz, Bolivia.

center of the sun twice its radius, as was about the case for the nearest star concerned in the British observations, the apparent displacement would simply be one half of the amounts just stated. In brief, the deflection would vary inversely as the distance.

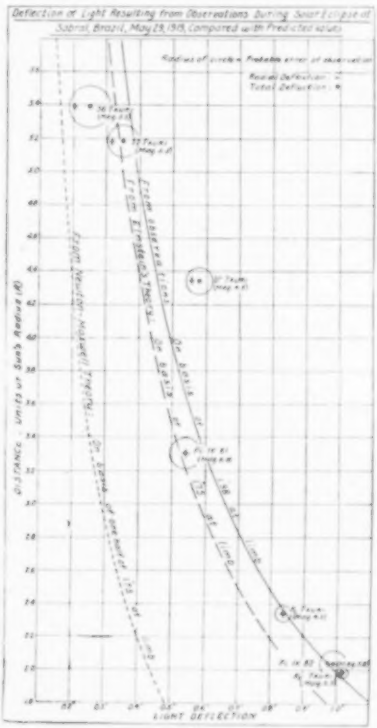
Now, according to the results from the best of the photographic plates obtained by the two British expeditions, one to Sobral, Brazil, and the other to the Île of Principe, west coast of Africa, the displacements of the stars accorded best with the predictions based on Einstein's later

theory. The British astronomers were exceedingly fortunate in having the opportunity of making the tests during an eclipse when there was a rich field of bright stars, the Hyades, sufficiently close to the sun, so that the quantities to be measured were well within the observational errors.

Professor W. W. Campbell's expedition made a test during the solar eclipse of June 8, 1918, at Goldendale, Washington, but, unfortunately, his observations had to be made on ninth magnitude stars, distant from the sun, two times or more the farthest star used by the British. His party was therefore obliged to work with much more minute quantities. The result from all of the observations was a mean displacement of 0″.05 in the right direction; but agreeing better with the predicted value (0″.08) on the basis of the Newtonian principles, than with that (0″.15) computed according to Einstein's later theory.

The combined result of the British and American tests would accordingly be that *light has weight*, besides exerting a measurable pressure; just how much weight depends upon whether the Newtonian or the Einstein principles are ultimately found correct. Astronomers are stimulated to make further tests in view of the important astronomical consequences of the Einstein theory; preparations by British astronomers are already under way for observations during the solar eclipse of September, 1922, which will occur in Australia, though the stellar conditions will not be as favorable as they were last year.

The importance of the subject has naturally caused some to advance possible other causes for the observed light deflections. Thus such an eminent solar physicist as Dr. H. F. Newall, of the Cambridge Observatory, while he is disposed to admit a possible effect according to Newtonian mechanics, prefers considering that the balance of the observed deflection of light is attributable to refraction in the solar atmosphere as mapped out, for example, to a certain extent by the solar corona.


Deflection of Light Resulting from Observations During Solar Eclipse at Sobral, Brazil, May 29, 1919, Compared with Predicted Values
Radius of Sun's Probable Error of Observation
Radial Deflection
Total Deflection
DISTANCE - UNITS OF SUN'S RADIUS (R)
LIGHT DEFLECTION

THE CHART, CONSTRUCTED BY THE DEPARTMENT OF TERRESTRIAL MAGNETISM, OF THE CARNEGIE INSTITUTION, gives a graphical representation of the law of variation with distance followed by the observed deflections for each star. Excepting the most distant star (56 Tauri), each star shows a deflection agreeing better with the Einstein value than with the Newton-Maxwell one. The probable error of observation is shown by the size of the circle around each star.

It would also appear, according to Dr. Bauer's investigations, that the observed deflections are not strictly radial, *i.e.*, not wholly in the direction of the sun's radius, indicating that there are some effects superposed upon the simple Newton, or Einstein, effects. He finds that the

non-radial effects occur in a systematic manner and not accidentally as they would if they were purely observational errors. He also finds that for the same distance a star in the polar regions of the sun showed a somewhat larger displacement than one in the equatorial regions. The question is raised, among others, how completely it was possible in the British observations to eliminate differential refraction effects as the rays passed through the earth's atmosphere.

Dr. Bauer's expedition was one of several expeditions sent out by the Department of Terrestrial Magnetism of the Carnegie Institution of Washington to make geophysical observations, the data from which are proving of interest in the discussion of the possible disturbing effects on the observed deflections of light. He himself observed the memorable eclipse at Cape Palmas, Liberia, where totality lasted longer, 6 minutes and 33 seconds, than at any other accessible station. He characterizes this eclipse as the most magnificent one of the four he has thus far observed; not only was the corona beautifully and finely developed but also a striking crimson prominence appeared on the sun's southeast limb which shot up 100,000 miles and had a base of 300,000 miles.

Dr. Bauer concludes with reference to the observed light deflections that "the best attitude to take is that of open-mindedness and to let no opportunity pass by for further experimental tests," and that "one of the most satisfactory results has been the stimulus imparted to further research in many fields which is bound to bear fruit."

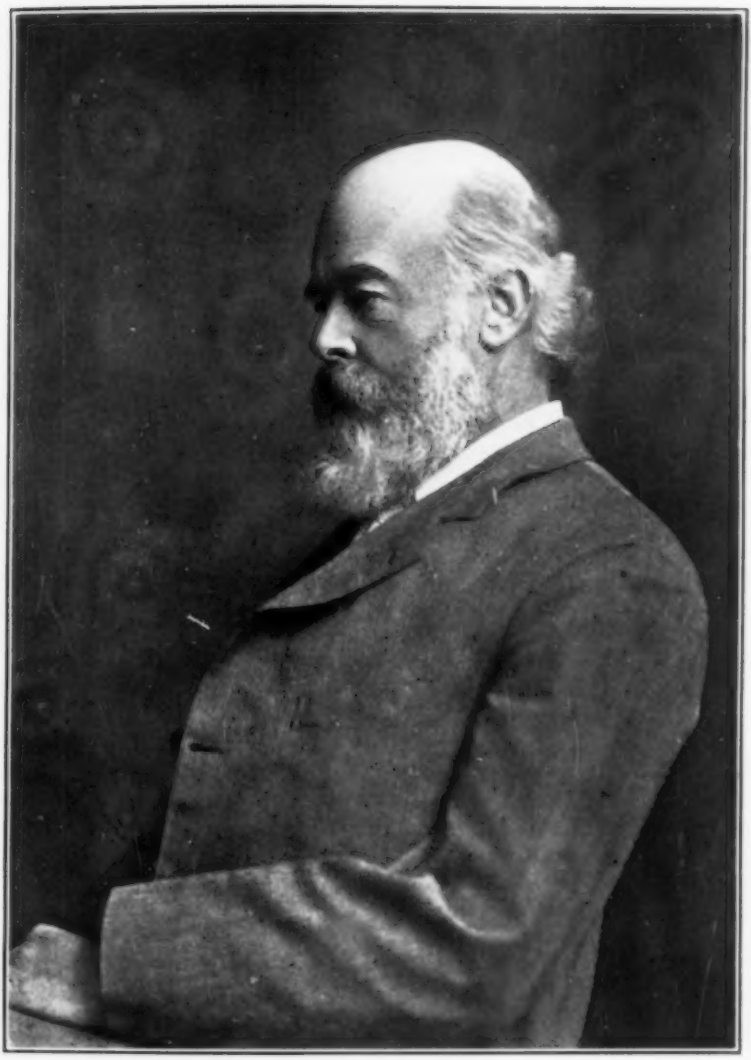
PROFESSOR EINSTEIN ON THE THEORY OF RELATIVITY

IN an article contributed to the London *Times*, Professor Albert Einstein has undertaken to present his theory of relativity in a form comprehensible to readers not trained to think in mathematical formulas. He calls attention to the fact that the ancient Greeks knew that the motion of a body must be described in reference to another body. In physics the bodies to which motions are spatially referred are termed systems of coordinates. The laws of mechanics of Galileo and Newton can be formulated only by using a system of coordinates.

The special relativity theory is the application of the following proposition to any natural process: "Every law of nature which holds good with respect to a coordinate system K must also hold good for any other system K' provided that K and K' are in uniform movement of translation." According to the Maxwell-Lorentz theory of electro-dynamics, however, light in a vacuum has a definite and constant velocity, independent of the velocity of its source.

These two principles have received experimental confirmation, but do not seem to be logically compatible. The special relativity theory achieved their logical reconciliation by making a change in kinematics, that is to say, in the doctrine of the physical laws of space and time. It became evident that a statement of the coincidence of two events could have a meaning only in connection with a system of coordinates, that the mass of bodies and the rate of movement of clocks must depend on their state of motion with regard to the coordinates.

But the older physics, including the laws of motion of Galileo and Newton, clashed with the relativistic kinematics. Physics had to be modified. The most notable change was a new law of motion for very rapidly moving mass-points, and this soon came to be verified in the case of electrically-laden particles. The most important result of the special relativity system concerned the iner-



SIR OLIVER LODGE.

Formerly professor of physics in the University of Liverpool and principal of the University of Birmingham.

mass of a material system. It became evident that the inertia of such a system must depend on its energy-content, so that we were driven to the conception that inert mass was nothing else than latent energy. The doctrine of the conservation of mass lost its independence and became merged in the doctrine of conservation of energy.

The special relativity theory which was simply a systematic extension of the electro-dynamics of Maxwell and Lorentz, had consequences which reached beyond itself. Although it may be necessary for our descriptions of nature to employ systems of coordinates that we have selected arbitrarily, the choice should not be limited in any way so far as their state of motion is concerned. This general theory of relativity was found to be in conflict with a well-known experiment, according to which it appeared that the weight and the inertia of a body depended on the same constants.

A generalized theory of relativity must include the laws of gravitation, and actual pursuit of the conception has justified the hope. But the way was harder than was expected, because it contradicted Euclidian geometry. In other words, the laws according to which material bodies are arranged in space do not exactly agree with the laws of space prescribed by the Euclidian geometry of solids. This is what is meant by the phrase "a warp in space." The fundamental concepts "straight," "plane," etc., accordingly lose their exact meaning in physics.

In the generalized theory of relativity, the doctrine of space and time, kinematics, is no longer one of the absolute foundations of general physics. The geometrical states of bodies and the rates of clocks depend in the first place on their gravitational fields, which again are produced by the material systems concerned.

Thus the new theory of gravitation diverges widely from that of Newton with respect to its basal principle. But in practical application the two agree so closely that it has been difficult to find cases in which the actual differences could be subjected to observation. As yet only the following have been suggested: (1) The distortion of the oval orbits of planets round the sun (confirmed in the case of the planet mercury). (2) The deviation of light-rays in a gravitational field (confirmed by the English Solar Eclipse expedition). (3) The shifting of spectral lines towards the red end of the spectrum in the case of light coming to us from stars of appreciable mass (not yet confirmed).

Professor Einstein says in conclusion: "The great attraction of the theory is its logical consistency. If any deduction from it should prove untenable, it must be given up. A modification of it seems impossible without destruction of the whole. No one must think that Newton's great creation can be overthrown in any real sense by this or by any other theory. His clear and wide ideas will for ever retain their significance as the foundation on which our modern conceptions of physics have been built."

SCIENTIFIC ITEMS

WE record with regret the death of Francis C. Phillips, for forty years professor of chemistry at the University of Pittsburgh; of Alfred J. Moses, professor of mineralogy in Columbia University; of Edwin A. Strong, emeritus professor of physics at the Michigan State Normal College; of Sir James Alexander Grant, the Canadian surgeon and paleontologist; and of two of the most distinguished German men of science, Wilhelm Pfeffer, the botanist of the University of Leipzig, and of Otto Bütschli, the zoologist of the University of Heidelberg.